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"Doubling-Down" for Defendants: The Pernicious Effects of Tort Reform

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“Doubling-Down” for Defendants: The Pernicious Effects of Tort Reform

Scott DeVito* & Andrew W. Jurs**

Abstract

Tort reform legislation developed as a response to a series of insurance crises and reactions that blamed the personal injury compensation system for those problems. Since measures of tort reform have been adopted, many researchers have analyzed their effects within and beyond the legal system, assessing how they affect damages, insurance claims, health costs, and physician supply.

Our study analyzes an underdeveloped area of research: the effect of tort reform on the filing of cases in court. Using two databases of state court filing data over 12 years, we examine how a damages cap for medical negligence claims affects case filings in the years immediately after its adoption. With several test states, we find that when a state adopts med mal damages caps, there is a statistically significant drop of 23 percent in med mal filings. We confirm this effect by also measuring the effect of a cap's nullification, and find that in the aftermath of a cap's removal case filings increase by 29 percent. Our work can therefore confirm and quantify the effect of damages caps on case filing.

Yet the finding is much more salient when we consider it in the context of a new and interesting study published in the *Journal of Empirical Legal Studies*. In their 2013 study, Myungho Paik, Bernard Black, and David Hyman found that filings of med mal torts have decreased in the last decade, not only in tort reform states *but also in states without it!* If so, our finding of a statistically significant drop in med mal filings in response to tort reform has a “doubling-down” effect:

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there is one reduction in filings due to tort reform, and also a background reduction in filings based on larger, non-statutory changes.

We believe that our findings regarding the effect of tort reform on med mal filings and the “doubling-down” effect significantly modify the cost-benefit analysis of tort reform. The positive impacts of tort reform have been significantly oversold, and the effects of tort reform disproportionately impact certain vulnerable citizens. If so, we believe that claimants are being doubly squeezed without significant public benefit. We therefore suggest that state legislators reconsider these efforts, or risk court intervention due to equal protection challenges.

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I. INTRODUCTION

Tort reform began as a response to insurance crises and health care providers' dissatisfaction with personal injury litigation and its method of "jackpot justice."¹ Whether the personal injury system was indeed the root cause of insurance crises remained largely untested prior to the adoption of tort reform.² Only after the adoption of tort reform legislation did researchers examine whether tort reform was necessary in the first instance.³ In addition, many also examined how tort reform measures—like limiting claimants' ability to initiate a lawsuit, increasing the standards for proof of claims, and decreasing available damages—result in effects both within and beyond the legal system.⁴ After examining the extensive research in the area, we offer our study in the most underdeveloped area of prior research: the effect of tort reform on the filing of cases at the state level.⁵ By examining the data, we can demonstrate that the effects of tort reform have been harsher than previously expected, and when that finding is combined with research testing the claimed benefits of tort reform, its cost-benefit balance is demonstrably off.

1. See, e.g., FRANK M. MCCLELLAN, *MEDICAL MALPRACTICE: LAW, TACTICS, AND ETHICS* 81 (1994); FRANK A. SLOAN & LINDSEY M. CHEPKE, *MEDICAL MALPRACTICE* 28 (2008); PAUL C. WEILER, *MEDICAL MALPRACTICE ON TRIAL* 12 (1991). As for the "jackpot justice" phraseology, see *Republican Platform 2000*, N.Y. TIMES, <http://bit.ly/1ce0oC1> (last visited Feb. 16, 2014) (denouncing "the trial lawyers' system of jackpot justice" and suggesting legislative reform). See generally *infra* Part II.A.

2. See SLOAN & CHEPKE, *supra* note 1, at 5 (dismissing the empirical foundation for arguments connecting tort liability to increased expenditures, noting that much of the public discourse is based on anecdotes); WEILER, *supra* note 1, at 12–14 (categorizing the connection of tort liability to problems with health care as "based largely on myth rather than fact"); Joanna M. Shepherd, *Products Liability and Economic Activity: An Empirical Analysis of Tort Reform's Impact on Businesses, Employment, and Production*, 66 VAND. L. REV. 257, 259–60, 291 (2013) (asserting that "there is a dearth of empirical evidence" exploring whether tort liability actually stifles economic activity in context of products liability reform, despite tort reform proponents' assertion of the connection). See generally *infra* Part II.A.

3. See, e.g., PATRICIA M. DANZON, *MEDICAL MALPRACTICE: THEORY, EVIDENCE AND PUBLIC POLICY* 19 (1985); PAUL C. WEILER ET AL., *A MEASURE OF MALPRACTICE* 70–71 (1993); Bernard Black et al., *Stability, Not Crisis: Medical Malpractice Claim Outcomes in Texas, 1988–2002*, 2 J. EMPIRICAL LEGAL STUD. 207, 208 (2005) (noting proponents of tort reform adhere to the theory that "[m]ed mal liability is the disease, insurance rate spikes are the symptoms"); Neil Vidmar, *Medical Malpractice and the Tort System in Illinois*, 93 ILL. B.J. 340 (2005); see also *infra* Parts II.B.3, IV.B.

4. See MCCLELLAN, *supra* note 1, at 81; WEILER, *supra* note 1, at 29–32; Glen O. Robinson, *The Medical Malpractice Crisis of the 1970's: A Retrospective*, 49 LAW & CONTEMP. PROBS. 5, 21–26 (1986); see also *infra* notes 40–46 and accompanying text.

5. Regarding the difficulty in obtaining data to evaluate the effect of tort reform on case filings, see Theodore Eisenberg, *The Empirical Effects of Tort Reform* 15 (Cornell Legal Studies Research Paper No. 12-26, 2012), available at http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2032740.

To analyze the effects of tort reform, we started by assembling two databases: a multi-state database of 12 years of state court filing data, and a state-level database of tort filings.⁶ We then measured how a particular type of tort reform measure—medical malpractice damages caps—affected the filing of medical malpractice cases. When we used our multi-state database to compare the adoption of a damages cap, in North Dakota in 1995 and Florida and Mississippi in 2003, to eight states that did not have medical malpractice caps,⁷ we found that adoption of a damages cap lowered the probability of filing a medical malpractice tort case by a statistically significant margin of 23 percent.⁸ Similarly, when we compared Florida and Mississippi, each of which adopted a medical malpractice cap in 2003, to Colorado, Missouri, and North Dakota, each of which had a damages cap during the relevant period, we found that adoption of a damages cap lowered the probability of filing a medical malpractice tort case by a statistically significant margin of 13 percent.⁹ Not only can we show a reaction to the initiation of the damages cap, but we can also confirm an effect by offering a new analysis not previously contained in the literature, namely, the effect of nullification of a damages cap. Using the state-level database, we analyzed the effect of a court's nullification of Alabama's damages cap in 1995, and found that the removal of a cap resulted in a statistically significant 29 percent increase in court filings.¹⁰

Clearly the adoption or removal of damages caps affects the filing of medical malpractice tort cases. This finding is not isolated, because if we also consider a new finding from a recent study in the *Journal of Empirical Legal Studies* (*JELS*), our results have quantified a new and interesting phenomenon: the “doubling-down” effect of tort reform.¹¹ In the *JELS* study, Myungho Paik and his colleagues found that filings of medical malpractice torts have decreased in states both with and without tort reform.¹² Because all states are experiencing a drop in filings, our analysis showing a statistically significant reduction in filings in states with tort reform, as compared to non-reform states, demonstrates that plaintiffs in reform states are experiencing two separate effects. First, plaintiffs are less likely to file a med mal tort regardless of tort reform

6. Regarding the methodology of assembling the datasets, see *infra* Part III.A.

7. The comparison (or “control”) states were: Arizona, Minnesota, Nevada, New Hampshire, New Jersey, New York, Oregon, and Wyoming. None of these states had medical malpractice caps in place during the relevant period. See *infra* Part III.A.3.a.

8. See *infra* Part III.B.

9. See *infra* Parts III.A.3.b, III.B.

10. See *infra* Parts III.A.3.c, III.B.

11. Myungho Paik et al., *The Receding Tide of Medical Malpractice Litigation: Part I—National Trends*, 10 J. EMPIRICAL LEGAL STUD. 612, 612 (2013).

12. *Id.* at 625 tbl.2; see also *infra* notes 84–86 and accompanying text.

due to a background reduction of overall filings. Second, plaintiffs in tort reform states are less likely to file med mal torts due to the effect of tort reform. Essentially, there are two separate and distinct forces causing med mal filings to shrink, depicted as follows:

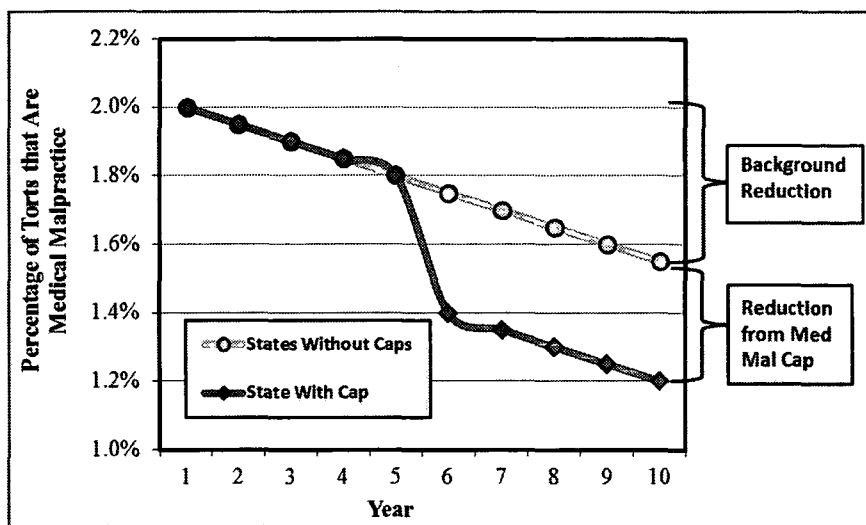


FIGURE 1. Combined effect of med mal caps and declining baseline

Considering the “doubling-down” effect and the larger state of empirical research in the area of tort reform, we believe that the cost-benefit analysis of damages caps and other reform measures changes significantly.¹³ Proponents’ claims of positive benefits to tort reform have been greatly oversold, while the negative effects of the caps have increased. Since that is the case, we believe state legislators must reconsider tort reform, and in so doing, reject the “silver bullet” theory of a cure-all for problems with tort litigation. Otherwise, legislators can continue along the tort reform path, knowing the full effects of their legislation and risking court intervention to overturn their measures on equal protection grounds.¹⁴

We begin the analysis of these issues in Part II with the historical story of the development of tort reform, why it began, and what different measures have been considered. Additionally, Part II examines the recent empirical research in the area of tort reform, including its effects

13. See *infra* Parts IV.A–B.

14. Regarding the equal protection concerns of tort reform, see *infra* notes 271–79 and accompanying text.

within the legal system—on damages awards, access to counsel, and case filings—and its effects beyond the legal system—on the number of insurance claims, the size of those claims, insurance premiums, medical expenditures, and physician supply.

We then begin our empirical analysis in Part III. We first explain the methodology of our study, including the construction of our two datasets for analysis. Next, we examine how case filings react to the adoption or nullification of damages caps in med mal cases, finding the aforementioned 13 percent to 23 percent decrease in filings after adoption of a cap, and 29 percent increase after its demise. Both of these findings demonstrate a direct connection between the adoption of damages caps and plaintiffs' filings in court.

In Part IV, we examine the implications of our findings in the context of the larger debate over tort reform. First, we discuss how, due to the background reduction in filings even in states without tort reform, a statistically significant effect of tort reform “doubles-down” the impact on plaintiffs. Then, we consider this finding in the larger picture of research in the area and perform a cost-benefit analysis of tort reform, finding that its benefits have been greatly oversold, and that its negative impacts are increasing. As a consequence, we urge state legislators to reconsider their tort reform measures, or risk court intervention to overturn them.¹⁵

By measuring actual court filing data in many states over more than a decade, this study quantifies the effect of damages caps on plaintiffs' filings, finding a direct effect of reforms on plaintiff case filings. Considering this finding in the context of other recent research in tort reform, we believe the negative consequences of tort reform are becoming more apparent, and suggest legislators reconsider those methods moving forward.

II. TORT REFORM—JUSTIFICATION, ADOPTION, AND RESEARCH ON ITS EFFECTS

Tort reform encompasses a variety of legislative changes affecting plaintiffs considering personal injury lawsuits, including: the substantive and procedural burdens of filing, the burden of proof and causation at trial, and the available recovery. In this Part, we will review why legislatures adopted tort reform, when the changes to the law took place, and what current research exists to measure the effects of reform.

15. See *infra* Part IV.C.

A. Justification and Adoption

Proponents of change in personal injury litigation have offered tort reform proposals as their response to an out-of-control judicial system and to spiraling costs. While a complete historical analysis of tort reform and its origin, development, and effects is beyond the scope of this Article, a brief history of the justifications for reform is in order. To ease the discussion, it helps to consider these reforms as three separate epochs: the 1970s, the 1980s and the early 2000s.¹⁶ In each period, the issue of tort reform became hotly debated, although not always for the same reasons.

In the 1960s, malpractice claims rose sharply, possibly as much as six-fold,¹⁷ and the malpractice insurance industry had to respond. Many simply abandoned the business of medical negligence insurance or threatened to do so.¹⁸ With fewer remaining carriers and claims increasing into the decade of the 1970s, insurance rates for physicians climbed steeply.¹⁹ Some studies indicate that insurance premiums rose by 600 percent for lower-risk specialties or insureds, but by 900 percent or more for higher-risk policies.²⁰ Physicians paying the premiums and feeling dissatisfied with the increased costs looked for explanations that justified such steep increases.

A survey conducted in 1971 captured the attitudes of physicians toward the personal injury compensation system.²¹ The physician responses indicated a deep distrust and antipathy toward attorneys and the compensation system for injuries. Of all physicians, 77 percent believed that the medical malpractice situation was "worse now than ever before," and nearly 47 percent of the doctors believed it was for a reason outside the medical profession.²² Of the 873 doctors surveyed, 26 percent believed aggressive lawyers were the reason for more common

16. See Robinson, *supra* note 4, at 7–8; see also SLOAN & CHEPKE, *supra* note 1, at 28.

17. WEILER, *supra* note 1, at 26 (citing Mark C. Kendall, *Expectations, Imperfect Markets, and Medical Malpractice Insurance*, in THE ECONOMICS OF MEDICAL MALPRACTICE 167, 176 (Simon Rottenberg ed., 1978)).

18. See Robinson, *supra* note 4, at 8–9; see also BARBARA WERTHMANN, MEDICAL MALPRACTICE LAW: HOW MEDICINE IS CHANGING THE LAW 127 (1984).

19. WEILER, *supra* note 1, at 27; WERTHMANN, *supra* note 18, at 127.

20. See Robinson, *supra* note 4, at 8 (citing Mark Kendall & John Haldi, *The Medical Malpractice Insurance Market*, in U.S. DEP'T OF HEALTH, EDUC., & WELFARE, REPORT OF THE SECRETARY'S COMMISSION ON MEDICAL MALPRACTICE app. at 494, 541 (1973)). During roughly the same period, Robinson notes that hospital insurance rates rose 750% between 1965 and 1973. Robinson, *supra* note 4, at 8 (citing MICHAEL T. SUMNER, THE DOLLARS AND SENSE OF HOSPITAL MALPRACTICE INSURANCE 19 (1979)).

21. William R. Pabst, *A Medical Opinion Survey of Physicians' Attitudes on Medical Malpractice*, in U.S. DEP'T OF HEALTH, EDUC., & WELFARE, *supra* note 20, app. at 83.

22. *Id.* app. at 85.

malpractice suits.²³ Finally, when asked how best to alleviate problems with malpractice, the most common response—from 26 percent of the respondents—was to adopt “laws limiting such suits,” while an additional 14 percent chose “reduced court judgments” as the solution.²⁴

The survey suggests that, in the face of rising malpractice insurance premiums, the medical community had found their culprit—they blamed lawyers.²⁵ In his analysis of the development of tort reform, Paul Weiler wrote:

[Doctors'] familiar refrain, echoed by many pundits and politicians, is that tort costs have soared because patient attorneys seeking hefty contingent fees are filing too many spurious allegations of medical negligence, and because unsophisticated juries, moved by the plight of often seriously disabled plaintiffs, too often give in to the temptation to use the doctor's insurer to award huge damage sums as redress for the patient's needs, irrespective of whether there is any tangible evidence of fault on the part of the doctor.²⁶

Something had to be done to rein in the personal injury lottery, and so the medical community looked to state legislatures for help.

State legislatures responded to the lobbying efforts of the medical community, fearing abandonment by health care providers if they did not.²⁷ Broadly speaking, reforms would include changes to the initiation of claims, the modifications for standards of proof at trial, and the limitation of recovery.²⁸ Plaintiffs would now face serious impediments blocking the path to recovery, from changes in the statute of limitations at the start to the limitation of damage recovery after trial.²⁹ The first cycle of tort reform had run its course.

23. *Id.* app. at 84.

24. *Id.* app. at 85.

25. The preface to a 1975 symposium from Duke Law Journal captures the moment: The term “medical malpractice” has become an increasingly frightening one to patients, doctors, and insurers as well. In recent months, the spectre of physician strikes, astronomical damage awards, soaring liability insurance premiums, and allegations of poor-quality medical care have stirred debate in state legislatures, in Congress, in the press, and in scholarly journals. The medical malpractice crisis is real, and the problems which created that crisis remain with us.

Symposium on Medical Malpractice, 1975 DUKE L.J. 1177, 1177 (cited in SLOAN & CHEPKE, *supra* note 1, at 309); see also SLOAN & CHEPKE, *supra* note 1, at 85; WEILER, *supra* note 1, at 11–12, 27; Robinson, *supra* note 4, at 14.

26. WEILER, *supra* note 1, at 12.

27. MCCLELLAN, *supra* note 1, at 79; WEILER, *supra* note 1, at 27; WERTHMANN, *supra* note 18, at 127.

28. See Robinson, *supra* note 4, at 21–26.

Following the reforms of the 1970s, tort reform remained dormant until a new crisis began in the mid-1980s. The crisis once again began with insurance cost concerns, rising out of premium increases spiking in the period from 1984–1987.³⁰ Physicians again responded by lobbying for limitations on personal injury liability, and the state legislatures obliged with a second wave of tort reform measures. In contrast to the 1970s, however, the 1980s also saw a political aspect of the crisis. Responding to the effect the insurance crisis had on business interests, the Reagan administration created a Tort Policy Working Group ("Study Group") led by the Attorney General to study the issue. Consisting of officials from the Department of Commerce, the Small Business Administration, and the Department of Justice,³¹ the Study Group's report laid blame squarely on an out-of-control personal injury system by stating: "while there are a number of factors underlying the insurance availability/affordability crisis, tort law is a *major cause* which the federal government can address"³² The Study Group recommended a series of tort reform measures that echoed the earlier reforms of the 1970s, namely, curtailing the filing of claims, increasing the standards of proof for trial, and limiting the collection of damages.³³ Tort reform had again been the response to an insurance crisis.

By the 2000s, the political aspects of tort reform had become a major driving force behind legislative change. In the 2000 platform of the Republican Party, the party blamed "the trial lawyers' system of jackpot justice" as a problem requiring major legislative reform.³⁴ Once President Bush assumed office, the platform had a powerful proponent.³⁵ Yet it was only after a new crisis in insurance that the platform gained

29. For a more detailed examination of the types of reform measures, and their adoption cycles, see *infra* Part II.A, notes 40–46 and accompanying text (discussing different measures, and different eras of state adoption of same).

30. SLOAN & CHEPKE, *supra* note 1, at 28 (citing *Property/Casualty Insurance Cycle*, INS. INFO. INST., <http://bit.ly/1nHfmCD> (last visited Feb. 17, 2014)).

31. TORT POLICY WORKING GRP., U.S. DEP'T OF JUSTICE, REPORT OF THE TORT POLICY WORKING GROUP ON THE CAUSES, EXTENT AND POLICY IMPLICATIONS OF THE CURRENT CRISIS IN INSURANCE AVAILABILITY AND AFFORDABILITY 1 (1986) [hereinafter TORT POLICY STUDY GROUP], available at <http://1.usa.gov/1moK8EZ>.

32. *Id.* at 5 (emphasis added); see also *Product Liability Reform Proposals: Hearing Before the Subcomm. on the Consumer of the S. Comm. on Commerce, Sci. & Transp.*, 99th Cong. 25–29 (1986) [hereinafter Meese Statement] (statement of The Honorable Edwin Meese III, Att'y Gen. of the United States).

33. TORT POLICY STUDY GROUP, *supra* note 31, at 4; Meese Statement, *supra* note 32, at 27–28; see also S.Y. TAN, MEDICAL MALPRACTICE: UNDERSTANDING THE LAW, MANAGING THE RISK 266 (2006).

34. See *Republican Platform 2000*, *supra* note 1.

35. See Alan G. Williams, *The Cure for What Ails: A Realistic Remedy for the Medical Malpractice "Crisis"*, 23 STAN. L. & POL'Y REV. 477, 482 (2012).

significant momentum on the issue.³⁶ While republicans in the House passed tort reform in 2004,³⁷ multiple states—mainly, but not exclusively, Republican-led states—also adopted significant reforms.³⁸ Once again, a crisis in insurance, this time with a political component, led to legislative reform limiting tort recovery through litigation.

In examining this historical story, the three eras of tort reform all grew out of the desire to limit the filing and recovery of lawsuits in order to lessen insurance premiums.³⁹ The specific reforms adopted demonstrate how these goals translated into legislation. Some legislation directly addressed the ability of a claimant to initiate a lawsuit. One type of legislation required an initial screening of any claim of medical malpractice by a qualified expert or expert panel.⁴⁰ Other states changed the applicable statute of limitations for filing of a case alleging medical negligence.⁴¹ While these reforms target the procedural aspects of the claimant's suit, another significant reform targeted the structural aspect of the litigation: the ability to get a lawyer through limitations on contingency fee agreements.⁴² Other reforms addressed the standards used in court to prove a claim once it has been filed. Some states required experts to attest to the standard of care in the particular community in question, or in a similar locale, thus limiting the availability of some experts.⁴³ Others limited or eliminated the doctrine of *res ipsa loquitor*, a controversial and limited way to avoid proof of breach, or challenged the rules for informed consent.⁴⁴

36. See James Dao, *A Push in States to Curb Malpractice Costs*, N.Y. TIMES, Jan. 14, 2005, at A21; see also SLOAN & CHEPKE, *supra* note 1, at 28 (citing *Property/Casualty Insurance Cycle*, *supra* note 30).

37. Help Efficient, Accessible, Low-Cost, Timely Healthcare (HEALTH) Act of 2004, H.R. 4280, 108th Cong. (2004). The same bill also passed in 2003. Help Efficient, Accessible, Low-Cost, Timely Healthcare (HEALTH) Act of 2003, H.R. 5, 108th Cong. (2003). Both failed in the Senate. See *H.R. 5 (108th)*, GOVTRACK, <http://www.govtrack.us/congress/bills/108/hr5> (last visited Feb. 17, 2014); *H.R. 4280 (108th)*, GOVTRACK, <http://www.govtrack.us/congress/bills/108/hr4280> (last visited Feb. 17, 2014).

38. Dao, *supra* note 36.

39. MCCLELLAN, *supra* note 1, at 81 (noting similarity of goals for different waves of reform).

40. MCCLELLAN, *supra* note 1, at 81; WEILER, *supra* note 1, at 29. See generally Ronen Avraham, *Database of State Tort Law Reforms (DSTLR 4th)* (Univ. of Tex. Sch. of Law, Law & Econ. Research Paper No. 184, 2011), available at http://papers.ssrn.com/sol3/papers.cfm?abstract_id=902711.

41. WEILER, *supra* note 1, at 28; Robinson, *supra* note 4, at 21–22.

42. WEILER, *supra* note 1, at 28–29; Robinson, *supra* note 4, at 22.

43. WEILER, *supra* note 1, at 30; Robinson, *supra* note 4, at 23.

44. WEILER, *supra* note 1, at 30; Robinson, *supra* note 4, at 24.

Finally, even if a claimant successfully filed a claim and then proved the necessary elements at trial, the state would limit the availability of damages using a variety of measures. Some statutes simply cap the damages available in any claim of medical malpractice, while others cap only the noneconomic component of the damages.⁴⁵ More limited reforms involved the limitation of joint and several liability, the offsetting of damages due to payment from a collateral source, or the elimination of lump-sum payments of damage awards.⁴⁶ The reformers believed these reforms collectively would provide a legislative solution to the insurance crisis by limiting or eliminating one of its primary causes.

When one examines when these reforms have been adopted, the cyclical nature of the tort reform movement becomes apparent. If we look at when states adopted damages caps in medical malpractice cases—the tort reform measure we will examine in Part III—the three eras of tort reform seem quite clear.⁴⁷

1975	CA, LA, OH
1976	IN, MD, NE, SD
1977	VA
1978	NM
1986	MA, MI, WV, MO
1987	AL
1988	CO
1995	MT, ND
2002	OH
2003	FL, MS, TX, OK
2004	NV

45. MCCLELLAN, *supra* note 1, at 81; WEILER, *supra* note 1, at 32; Robinson, *supra* note 4, at 25–26.

46. MCCLELLAN, *supra* note 1, at 81; WEILER, *supra* note 1, at 31–32; Robinson, *supra* note 4, at 26.

47. For a comprehensive list of the adoption of various tort reform measures, see generally Avraham, *supra* note 40. The dates of each of the medical malpractice caps in Table 1, except for North Carolina, are contained therein. *Id.* Avraham’s database is only current through 2010. *Id.* As such, the North Carolina statute in the chart, adopted in June 2011, is not contained therein. *Id.*; see N.C. GEN. STAT. ANN. § 90-21.19 (West, Westlaw through S.L. 2013-257 of the 2013 Reg. Sess.). Note also that several states do not neatly fit the “three era” pattern, as North Dakota and Montana adopted medical malpractice caps in 1995, and North Carolina did in 2011. Avraham, *supra* note 40.

2005	AK, CT, SC, GA, IL
2006	WI
2011	NC

TABLE 1. Adoption of damages caps in medical malpractice cases, by year of adoption

In sum, the tort reform movement of the late twentieth and early twenty-first century was a direct response to cyclical insurance crises of the same era: a result of blaming the personal injury system for those crises, and then responding with legislation intended to curb the excesses of the tort system.

B. Empirical Analysis of the Effects of Tort Reform

Beyond demagoguery, it is clear that proponents of tort reform did not have specific data or research to support their legislative agenda.⁴⁸ Many researchers have sought to empirically test the effect of tort reform legislation, however, and these studies can be further broken down into different groupings based on varying methodologies.⁴⁹ For purposes of this summary, the types of studies will be summarized based on the primary effect being within or outside the legal system.

1. Effects Within the Legal System

When assessing the effect of tort reform on the legal system, researchers use three factors to measure the effect: the damage awards given by juries, access to attorneys, and the filing of cases.

Damage award analysis measures whether tort reform has achieved its intended goal of reducing payouts in personal injury litigation. Even with this relatively simple metric, however, results are quite varied. In their 2004 study of two decades of medical malpractice verdicts reported in California, David Studdert, Tony Yang, and Michelle Mello discovered that noneconomic damages caps lowered total damage awards by 34 percent, and the noneconomic portion of damages by 73 percent.⁵⁰ Even with that clear result, however, Studdert et al. also found that noneconomic damages caps had enormous variations in their effects,

48. In fact, the existing research—as we will see *infra*—remains at best inconclusive, even now. Eisenberg, *supra* note 5, at 31.

49. *Id.* at 1.

50. David M. Studdert et al., *Are Damages Caps Regressive? A Study of Malpractice Jury Verdicts in California*, 23 HEALTH AFF. 54, 58 (2004).

drastically reducing recoveries for more seriously injured claimants.⁵¹ The same year, a second study—this time by the RAND Institute for Civil Justice—analyzed California medical malpractice damage awards using verdicts reported in a statewide trade publication.⁵² In their analysis, the authors found a 30 percent reduction in total damages awarded after a plaintiff's verdict due to the noneconomic damages cap.⁵³ They noted, however, that these reductions disproportionately affect two categories of plaintiffs: those with severe injuries such as brain damage or paralysis, and female claimants.⁵⁴ A third study in 2004 regarding medical malpractice verdicts took a close look at damages caps' disproportionate impact on certain plaintiffs.⁵⁵ Examining jury verdicts in California, Florida, and Maryland, Lucinda Finley found that caps reduce verdicts to women and the elderly in a statistically significant way.⁵⁶ To Finley, the disproportionate impact of damages caps is a clear form of discrimination, inhibiting equal access to justice.⁵⁷ A study performed the next year by Catherine Sharkey measured awards of compensatory damages after many tort reform measures, including noneconomic damages caps, were enacted.⁵⁸ Using her sample of actual case data from 1992 to 2001,⁵⁹ Sharkey found that if one controls for the severity of the injury incurred,⁶⁰ a noneconomic damages cap has no statistically significant effect on compensatory damages in personal injury cases.⁶¹ In 2009, David Hyman and his colleagues also analyzed the issue of disproportional impacts of damages caps on certain claimants using data from medical malpractice cases in Texas.⁶² The researchers found that a noneconomic damages cap reduces the overall recovery by

51. *Id.* at 58–59 exhibit 3.

52. NICHOLAS M. PACE ET AL., RAND INST. FOR CIVIL JUSTICE, CAPPING NON-ECONOMIC AWARDS IN MEDICAL MALPRACTICE TRIALS: CALIFORNIA JURY VERDICTS UNDER MICRA, at xix (2004), available at <http://bit.ly/N3oLZH>.

53. *Id.* at xx.

54. *Id.* at xxiii.

55. Lucinda M. Finley, *The Hidden Victims of Tort Reform: Women, Children, and the Elderly*, 53 EMORY L.J. 1263 (2004).

56. *Id.* at 1313.

57. *Id.* at 1265, 1314.

58. Catherine M. Sharkey, *Unintended Consequences of Medical Malpractice Damages Caps*, 80 N.Y.U. L. REV. 391 (2005).

59. *Id.* at 446–48.

60. Sharkey also finds that severity of injury does have an effect on recovery, as one might expect because the injuries sustained require additional compensation. *Id.* at 470–72.

61. *Id.* at 469, 473, 478. This result is not necessarily at odds with the previous studies, in that many of them find disproportionate impacts due to severity of injury. For Sharkey, once that severity of injury is accounted for, the noneconomic damages cap has no statistically significant effect. *Id.* at 469.

62. David A. Hyman et al., *Estimating the Effect of Damages Caps in Medical Malpractice Cases: Evidence from Texas*, 1 J. LEGAL ANALYSIS 355 (2009).

27 percent,⁶³ although the reductions were significantly larger for the elderly or claimants filing based on a fatal injury.⁶⁴ These studies collectively indicate that a noneconomic damages cap can decrease recoveries for injured plaintiffs, but that the effect may disproportionately impact certain groups, namely, the more seriously injured, the elderly, and women.⁶⁵

Measured reductions in damages may have an impact on injured claimants' access to counsel, and several studies indicate the problem may be severe. In the 2004 RAND study of tort reform in California, the researchers examined the effect of tort reform—both noneconomic damages caps and also direct contingency fee limitations—on attorneys' fee recovery in personal injury litigation that went to trial.⁶⁶ Measuring each limitation separately, the RAND study found that the noneconomic damages cap reduced overall attorneys' fees by 30 percent, while the contingency fee limitations reduced fees by 46 percent.⁶⁷ When examined together, overall fees in the sample group had been slashed 60 percent due to tort reform in California.⁶⁸ In a study of tort reform on attorneys' willingness to take cases, Stephen Daniels and Joanne Martin surveyed over 500 plaintiffs' attorneys whose contingency fee recoveries accounted for a significant portion of their business.⁶⁹ Among plaintiffs' attorneys, a significant portion believes that Texas's tort reform measures in the area of medical malpractice have had a negative effect on their practice.⁷⁰ They also discovered that tort reform formed one part of changes to the broader market environment, including negative advertising and jury perceptions of plaintiffs.⁷¹ They conclude that "in Texas, tort reform succeeded by changing the market environment in which plaintiffs' lawyers operate, making it harder, in the lawyers' estimation, to stay profitable."⁷² This has an enormous impact on the operation of the law because "[w]ithout lawyers willing to take an injured party's case, the law has no teeth. It provides remedies in theory

63. *Id.* at 380.

64. *Id.* at 381–82.

65. For further discussion of the issue of disproportionate impacts of tort reform, see *infra* Part IV.B.

66. PACE ET AL., *supra* note 52, at xxiii–xxv, 35–41; see also *supra* text accompanying note 52.

67. PACE ET AL., *supra* note 52, at xxiv fig.S.1, 36–37.

68. *Id.* at xxiv fig.S.1, 37. Pace and his colleagues are careful to note that this result includes only trial awards, so it may overestimate the effect since it does not attempt to measure non-trial cases (settlements, informal claims, etc.). See *id.* at 37 n.2.

69. Stephen Daniels & Joanne Martin, *The Strange Success of Tort Reform*, 53 EMORY L.J. 1225, 1237 n.30 (2004).

70. *Id.* at 1239 tbl.2.

71. *Id.* at 1241 tbl.3, 1244 tbl.4.

72. *Id.* at 1262.

only.”⁷³ A third study, by Steven Garber and others, has found similar results in the area of medical malpractice.⁷⁴ By analyzing the survey responses of nearly a thousand plaintiffs’ attorneys, Garber found that the likelihood that an attorney takes a case decreases in the event the case is subject to tort reform.⁷⁵ In fact, when Garber examined the likelihood of an attorney taking a case subject to tort reform in light of the expected dollar amount of the case, the likelihood of an attorney accepting a high-payoff case decreased *more* than for low-payoff cases in the event of tort reform, since those high-payoff cases are more likely to have significant damage reductions by caps.⁷⁶ Because of these effects, Garber concludes that tort reform measures limit “access to justice.”⁷⁷

The third major area of empirical assessment of tort reform measures the effect of reforms on filing rates, attempting to quantify whether the legislation meets the intended goal of reducing the incidence of personal injury litigation.⁷⁸ A caveat is in order, however; Professor Theodore Eisenberg and others have found that in the area of filing rates, the difficulty in obtaining case-specific datasets has limited the number of studies.⁷⁹ Of those studies that do measure the effects of tort reform, results are varied. In 1999, Mark Browne and Robert Puelz performed a multi-state study analyzing the effect of noneconomic damages caps on automobile-related personal injury lawsuits.⁸⁰ By analyzing a dataset of insurance claims, which included whether a case had been filed in court, Browne and Puelz found that noneconomic damages caps reduced the probability of filing of a claim by 65 percent.⁸¹ In her single-state study in 2008, Patricia Hatamyar measured how comprehensive tort reform in

73. *Id.*

74. Steven Garber et al., *Do Noneconomic Damages Caps and Attorney Fee Limits Reduce Access to Justice for Victims of Medical Negligence?*, 6 J. EMPIRICAL LEGAL STUD. 637 (2009).

75. *Id.* at 677.

76. *Id.* at 677.

77. *Id.* at 682.

78. See *supra* text accompanying notes 28–29, 32–33.

79. Eisenberg, *supra* note 5, at 15; see also CONG. BUDGET OFFICE, THE EFFECTS OF TORT REFORM: EVIDENCE FROM THE STATES 9 (2004), available at <http://1.usa.gov/1ci39SK>. On the other hand, there are significantly more studies measuring the effect of tort reform on claim frequency. See *infra* Part II.B.2. For our analysis, we did collect and evaluate a dataset, and we explain that process *infra* Part III.A.

80. Mark J. Browne & Robert Puelz, *The Effect of Legal Rules on the Value of Economic and Non-Economic Damages and the Decision to File*, 18 J. RISK & UNCERTAINTY 189 (1999).

81. *Id.* at 208 (finding the probability of filing drops from 4.0% to 1.4% when a noneconomic damages cap has been adopted).

Oklahoma in the early 2000s affected case filings.⁸² By looking at the rate of filing at the time of the reforms to four years later, Hatamyar found that the cap decreased case filings in the years afterward by 17.5 percent for all torts and by 29.3 percent for medical malpractice claims.⁸³ Finally, just this year Myungho Paik, Bernard Black, and David Hyman looked at multiple states to assess tort filing rates in the past decade.⁸⁴ Examining medical malpractice case filing rates in 18 states, the authors found that within all 18 there had been a collective 33.7 percent drop in filings.⁸⁵ While the paper lacks specific information comparing cap to non-cap states, a brief analysis of the numbers indicate a mean per-state reduction of 23.8 percent for the no-cap states, and 42.4 percent for the cap states.⁸⁶

Researchers analyzing tort reform and the legal system have found significant changes arising from the implementation of various legislative enactments. Damages caps appear to succeed in reducing the damages of claimants, but most likely do so disproportionately to certain groups such as women, the elderly, and the seriously injured. Those damage reductions appear to affect the economics of the contingency fee system for plaintiffs' counsel, affecting access to justice for potential plaintiffs. Finally, although studies on filing rate are the least developed area of research, they may show case filing reductions attributable to tort reform measures, although some drop may not be attributable to legislative changes at all.⁸⁷

2. Other Effects

Of course, tort reform has significant effects that never reach the legal system. A second series of studies has addressed the effect of tort

82. Patricia W. Hatamyar, *The Effect of "Tort Reform" on Tort Case Filings*, 43 VAL. U. L. REV. 559 (2009).

83. *Id.* at 561–62.

84. Paik et al., *supra* note 11, at 613.

85. *Id.* at 625 tbl.2.

86. The idea of a background drop in case filings is also supported by a recent study by the Court Statistics Project at the National Center for State Courts. Reviewing the caseload statistics of the 13 states with available data, Cynthia Lee and Robert LaFountain found that between 1999 and 2008, medical malpractice filings dropped 15%. CYNTHIA G. LEE & ROBERT C. LAFOUNTAIN, COURT STATISTICS PROJECT, MEDICAL MALPRACTICE LITIGATION IN STATE COURTS 1, 3 (2011), available at <http://bit.ly/1nHxrK3>. A 2012 update covers the period from 2001 to 2010, and finds the trend continues; between 2001 and 2010, state court filings for medical malpractice cases declined 23%. *Medical Malpractice Caseloads Continue Prolonged Decline*, COURT STAT. PROJECT, <http://bit.ly/1gWyQAU> (last visited Feb. 17, 2014) [hereinafter *Court Statistics Project*].

87. Regarding the background reduction in tort filings, see Paik et al., *supra* note 11, at 625 tbl.2. See also *supra* text accompanying notes 84–86.

reform in these areas by measuring: the number and payment of claims by insurance, insurance premiums, medical costs, and the supply of physicians. In so doing, researchers have examined the broader implications of the tort reform movement, and whether tort reform has affected insurance markets as intended.⁸⁸

Without examining any effects on filing rates in court, several studies have assessed whether tort reform reduces the number of insurance claims for compensation. In his 2001 study, Albert Yoon measured the effect of tort reforms in Alabama on payment of insurance claims.⁸⁹ Yoon collected and analyzed a dataset including 13 years of medical negligence claims filed against health care providers in four states.⁹⁰ By comparing the tort reform state, Alabama, to three control states, Arkansas, Tennessee, and Mississippi, the data demonstrate that the implementation of a damages cap reduced overall payouts in Alabama by roughly \$20,000, and that this was a greater difference than the control states by a statistically significant margin.⁹¹ Yoon's analysis also shows that, following the nullification of Alabama's damages cap in 1995,⁹² average claim payouts increased significantly by about \$45,000 per claim, which was above the control state increase by a significant amount.⁹³ Paik, Black, and Hyman's analysis of tort reform in 2013 mirrors this result. In their analysis of a database of claims paid by physicians,⁹⁴ they found that the average payout per claim for large claims was 23 percent lower in cap states than in non-capped states, and that when some states adopted caps in the early 2000s, they dropped from the no-cap level to converge with older cap states' payout rates.⁹⁵ Their data on the average payout per physician demonstrated a 41 percent difference between cap states and no-cap states.⁹⁶ As with the analysis of payout per claim, when states adopted caps in the early 2000s, the payout per physician fell from the no-cap levels to converge with the older cap state payout rates.⁹⁷

88. See *supra* text accompanying notes 19, 30, 36.

89. Albert Yoon, *Damage Caps and Civil Litigation: An Empirical Study of Medical Malpractice Litigation in the South*, 3 AM. L. & ECON. REV. 199 (2001).

90. *Id.* at 209.

91. *Id.* at 216.

92. In 1995, the Alabama Supreme Court overturned the cap on damages for medical malpractice cases, finding it unconstitutional under state constitutional law. *Ray v. Anesthesia Assocs. of Mobile, P.C.*, 674 So. 2d 525, 526 (Ala. 1995).

93. Yoon, *supra* note 89, at 216–20. For our analysis of the effect of Alabama's nullification of damage caps on filing rates, see *infra* Part III.B.

94. Regarding the methodology of the study, see Paik et al., *supra* note 11, at 615–16.

95. *Id.* at 627 fig.7.

96. *Id.* at 628.

97. *Id.* at 627 fig.7 (Panel B).

Beyond the payment amounts, Paik, Black, and Hyman also analyzed the number of paid claims for capped and no-cap states. Using the same dataset of insurance claims, they found that for the number of claims filed, states with caps had an average of 25 percent fewer paid claims.⁹⁸ As with the damages amounts, when some states adopted damages caps in the 2000s, they fell from the higher no-cap rate of paid claims to converge with the lower old-cap rate.⁹⁹ The 25 percent decrease in the rate of paid claims for capped states is slightly larger than a 10 to 13 percent decrease in total claims found by Ronen Avraham in his 2007 study addressing the same issue.¹⁰⁰ Avraham studied the issue with a database of settled claims from the National Practitioners Data Bank, assessing these settlement claims in light of several types of tort reform.¹⁰¹ Not only did Avraham find a 10 to 13 percent decrease in the total number of settled claims due in states with damages caps; he also found a reduction in the payment-per-doctor amount of 15 to 20 percent.¹⁰² In a state-specific inquiry into the effect of tort reform on claims made, Leonard Nelson, Michael Morrissey, and Meredith Kilgore examined claims data in Mississippi, Louisiana, and Alabama contained within the National Practitioners Data Bank.¹⁰³ After a cap on damages in medical malpractice cases came into effect in 2003, Nelson et al. found a decline in the total payout on claims and the total claims made as compared to the other control states.¹⁰⁴ Their analysis showed a 38.5 percent decrease in claims, with a corresponding 31.7 percent drop in total payout.¹⁰⁵ The reductions in payout and number of claims indicate effects on the system of personal injury compensation, but a series of papers show some debate over whether these reductions translate to a corresponding reduction in insurance premiums for health care providers.¹⁰⁶ Of course, that was the intention of tort reform in the first instance.¹⁰⁷

98. *Id.* at 625.

99. Paik et al., *supra* note 11, at 626 fig.6.

100. Ronen Avraham, *An Empirical Study of the Impact of Tort Reform on Medical Malpractice Settlement Payments*, 35 J. LEGAL STUD. S183, S203 (2007).

101. Regarding the methodology of the study, see *id.* at S190. For the complete list of the tort reforms Avraham analyzed, see *id.* at S191.

102. *Id.* at S206. This compares to the 23% difference in payouts in large claims, and 41% difference in payout per physician, found by Paik et al. in their study of the issue. See Paik et al., *supra* note 11, at 627–28 fig.7; *supra* text accompanying notes 95–96.

103. Leonard J. Nelson III et al., *Medical Malpractice Reform in Three Southern States*, 4 J. HEALTH & BIOMEDICAL L. 69, 141 (2008).

104. *Id.* at 142.

105. *Id.* at 146 tbl.8.

106. For an overview of the research in the area, see Leonard J. Nelson III et al., *Damages Caps in Medical Malpractice Cases*, 85 MILBANK Q. 259, 267–69 (2007).

107. See *supra* text accompanying notes 17, 19, 24.

A series of older studies from the 1990s analyzed the effect of damages caps on insurance premiums, finding that caps reduce premiums by between 8.4 percent and 12.4 percent.¹⁰⁸ One study dissented. In a 1998 study using data from a longer period of time, Mark Paul Guis found that damages caps might not affect premiums after all.¹⁰⁹ The pattern of a majority of studies finding an effect, with some dissent, continues with the more modern studies. In a 2004 study, Danzon et al. found a 5.7 percent reduction in premiums for states with caps, and Kilgore et al. have found a significantly larger effect, between 17.3 percent and 25.5 percent, depending on specialty.¹¹⁰ A 2009 study by Patricia Born, W. Kip Vicusi, and Tom Baker also finds noneconomic damages caps reducing losses to insurance carriers,¹¹¹ and a 2010 study of insurance costs by Charles Ellington and his colleagues also found a reduction of 23.4 percent in hospital malpractice premiums (per bed) in states with noneconomic damages caps.¹¹² However, the studies are not uniform. In their 1999 analysis, J. Robert Hunter and Joanne Doroshow rejected the idea that tort reform brings down insurance rates.¹¹³ When they examined 14 years of insurance data, they found "no evidence that general, across-the-board 'tort reform' . . . has lowered insurance rates/loss costs."¹¹⁴ Specifically for medical malpractice-related tort

108. Nelson et al., *supra* note 106, at 268–69 (citing Daniel Kessler & Mark McClellan, *Do Doctors Practice Defensive Medicine?*, 111 Q. J. ECON. 353 (1996); W. Kip Vicusi & Patricia Born, *Medical Malpractice Insurance in the Wake of Liability Reform*, 24 J. LEGAL STUD. 463 (1995)). In addition, Nelson et al. cite a series of older studies that measure damages caps and other tort reform in aggregate, finding a 16.6% to 27.7% reduction in insurance premiums after adoption of reforms. *Id.* (citing W. Kip Vicusi et al., *The Effects of 1980s Tort Reform Legislation on General Liability and Medical Malpractice Insurance*, 6 J. RISK & UNCERTAINTY 165 (1993); Glenn Blackmon & Richard Zeckhauser, *The Effect of State Tort Reform Legislation on Liability Insurance Losses and Premiums* (1990) (unpublished manuscript)).

109. Mark Paul Guis, *Using Panel Data to Estimate the Determinants of Medical Malpractice Insurance Premiums*, 5 APPLIED ECON. LETTERS 37, 37 (1998) (cited in Nelson et al., *supra* note 106, at 269).

110. Nelson et al., *supra* note 106, at 268 (citing Patricia M. Danzon et al., *The "Crisis" in Medical Malpractice Insurance*, in BROOKINGS-WHARTON PAPERS ON FINANCIAL SERVICES 55 (Robert E. Litan & Richard Herring eds., 2004); Meredith L. Kilgore et al., *Tort Law and Medical Malpractice Insurance Premiums*, 43 INQUIRY 255 (2006)).

111. Patricia Born et al., *The Effects of Tort Reform on Medical Malpractice Insurers' Ultimate Losses*, 76 J. RISK & INS. 197, 209 (2009).

112. Charles R. Ellington et al., *State Tort Reforms and Hospital Malpractice Costs*, 38 J.L. MED. & ETHICS 127, 131 tbl.1 (2010) (displaying mean malpractice premiums of \$4,158 per bed in states with no caps, compared with mean of \$3,186 for states with caps).

113. J. ROBERT HUNTER & JOANNE DOROSHOW, CTR. FOR JUSTICE & DEMOCRACY, PREMIUM DECEIT: THE FAILURE OF "TORT REFORM" TO CUT INSURANCE PRICES 9 (1999), available at <http://bit.ly/1gWMuEe>.

114. *Id.* at 17.

reform, the authors found a counterintuitive result: that states with mid-range tort reforms had insurance premiums increase at a higher rate than states with no tort reform at all.¹¹⁵ Instead of tort reform changing insurance rates, they surmised that factors other than tort law must have driven any change.¹¹⁶

Insurance premium reductions had been one promised benefit of tort reform, but the research on whether that benefit has been achieved remains unclear. Proponents of tort reform also promised that it could result in lowered medical costs. Just as with the insurance premium issue, however, studies have yet to confirm tort reform does achieve that desired goal.

In the political climate of the 2000s, health care expenditure had become a hotly debated issue. By looking at 25 years of Medicare spending, the Congressional Budget Office (CBO) analyzed the effect of tort reform on health care spending in 2006.¹¹⁷ Once it controlled for changes to Medicare payment policies, the CBO concluded that noneconomic damages caps did not demonstrate a statistically significant effect on Medicare expenditures.¹¹⁸ The report goes so far as to suggest that there may be an “outside factor that may have caused changes in relative spending in states that passed tort limits, and that was correlated with the passage of tort limits.”¹¹⁹ More recent studies make similar conclusions. In a 2009 paper in the *Journal of Health Economics*, Frank Sloan and John Shadle also analyzed 25 years of Medicare data and agreed with the CBO report: “Direct reforms [including damages caps] did not significantly reduce payments for Medicare-covered services in any specification.”¹²⁰ In conclusion, they believe that “tort reforms do not significantly affect medical decisions, nor do they have a systematic effect on patient outcomes.”¹²¹ A more recent study by Myungho Paik and others addressed the same issue, examining Medicare spending in light of Texas’s comprehensive tort reform of 2003.¹²² When they compared high-risk to low-risk areas of Texas, they hypothesized that if tort reform does work, it would reduce Medicare spending in high-risk

115. *Id.* at 17.

116. *Id.* at 18.

117. CONG. BUDGET OFFICE, PUB. NO. 2668, MEDICAL MALPRACTICE TORT LIMITS AND HEALTH CARE SPENDING (2006), available at <http://1.usa.gov/1mp0cq5>.

118. *Id.* at 35.

119. *Id.* at 34.

120. Frank A. Sloan & John H. Shadle, *Is There Empirical Evidence for “Defensive Medicine”? A Reassessment*, 28 J. HEALTH ECON. 481, 488 (2009).

121. *Id.* at 481, 490.

122. Myungho Paik et al., *Will Tort Reform Bend the Cost Curve? Evidence from Texas*, 9 J. EMPIRICAL LEGAL STUD. 173 (2012).

counties due to physicians practicing less "defensive medicine."¹²³ Yet the data demonstrated that this hypothesis is incorrect. They found "no evidence that spending levels or trends in high-risk counties declined relative to low-risk counties and some evidence of *increased* physician spending in high-risk counties."¹²⁴ When Texas was compared to other states, Paik et al. could also find no evidence of a pattern of reduced spending after tort reform.¹²⁵ They concluded that tort reform is not the answer to reduce health care expenditure, and those who are "interested in a magic bullet that would limit the growth of health care spending should look elsewhere."¹²⁶

Because the data do demonstrate that health care expenditures do not change in response to tort reform, maybe the benefit can be captured elsewhere. Some researchers have looked beyond insurance and costs effects to examine another proposed benefit of tort reform: physician supply.¹²⁷ Yet when the studies are examined as a group, the success of tort reform at attracting additional health care professionals seems, at best, mixed.¹²⁸ One group of studies measuring the effect of tort reform on physician supply finds that damages caps do increase the availability of physicians between four and 12 percent.¹²⁹ Yet even in these studies, the effects of tort reform are much smaller than other, non-liability factors such as climate, the availability of residency programs, or even high HMO penetration.¹³⁰ In contrast to these studies, a second group of studies does not find that damages caps affect physician supply. In examining the effect of damages caps in periods of time between ten and 30 years, three separate studies were unable to find evidence that

123. *Id.* at 181–82.

124. *Id.* at 173 (emphasis added), 203.

125. *Id.* at 203.

126. *Id.* at 211.

127. Tort reform has always had a connection to the goal of ensuring a continued supply of health care providers. See *supra* text accompanying note 27.

128. For a complete list of studies, see Eisenberg, *supra* note 5, at 22–23 tbl.2.

129. FRED J. HELLINGER & WILLIAM E. ENCINOSA, U.S. DEP'T OF HEALTH & HUMAN SERVS., *THE IMPACT OF STATE LAWS LIMITING MALPRACTICE AWARDS ON THE GEOGRAPHIC DISTRIBUTION OF PHYSICIANS* (2003), available at <http://1.usa.gov/1e1pRLu> (finding "States with caps have about 12 percent more physicians per capita than States without a cap"); Jonathan Klick & Thomas Stratmann, *Medical Malpractice Reform and Physicians in High-Risk Specialties*, 36 J. LEGAL STUD. S121, S131 (2007) (finding between a 3.9% and 4.1% greater number of physicians in ten high-risk specialties, and 6.1% to 6.6% increase for the top five specialties, in states with damages caps). See generally Eisenberg, *supra* note 5, at 22 tbl.2.

130. HELLINGER & ENCINOSA, *supra* note 129, at 22 tbl.6 (finding 13.65 more physicians per 100,000 residents for caps compared to 18.87 for HMO, 169 for residency, and 60.5 for climate).

physicians were more likely to reside in states with tort reform.¹³¹ In the aggregate, the studies are mixed and do not clearly support the proponents' claim that tort reform would radically alter physician incentives.

3. Post-Hoc Analysis of the Necessity for Reform

Having reviewed recent studies of the effect of tort reform both on the legal system (examining damage awards, access to counsel, and filing rates) and on other markets (examining insurance markets, claims, premiums, medical costs and physician supply), we can now see that the studies at best provide mixed support for the claims of tort reform proponents. After looking at the same data, several groups of researchers began to fundamentally question whether tort reform was necessary in the first instance. Two studies in 2005 analyzed the issue. At the request of the Illinois Bar Association and in the context of a state debate over adoption of tort reform,¹³² Neil Vidmar studied the issue of whether the perceived "crisis" in tort litigation was real.¹³³ Vidmar's result is unequivocal: no data support the idea that insurance premiums were rising dramatically, and no data indicate an increase in filing of medical malpractice lawsuits.¹³⁴ By Vidmar's calculations, the physician supply had even increased, both in absolute numbers and in the ratio of physicians to the overall population.¹³⁵ Vidmar concluded: "The best data for Illinois that were available for this report indicate that juries are not to blame for the problems involving the increases in doctor's liability premiums. It is time to look for other causes of the ailment."¹³⁶ Despite the study, the Illinois legislature passed tort reform the same year.¹³⁷

A second 2005 study analyzing the arguments for tort reform examined the State of Texas in the years leading to the 2003 tort reforms

131. David A. Matsa, *Does Malpractice Liability Keep the Doctor Away? Evidence from Tort Reform Damage Caps*, 36 J. LEGAL STUD. S143, S145 (2007) (finding some effect in solely rural areas, but only for specialists); Charles Silver et al., *The Impact of the 2003 Texas Medical Malpractice Damages Cap on Physician Supply and Insurer Payouts: Separating Facts from Rhetoric*, TEX. ADVOC., Fall 2008, at 25, 27, 29; Y. Tony Yang et al., *A Longitudinal Analysis of the Impact of Liability Pressure on the Supply of Obstetrician-Gynecologists*, 5 J. EMPIRICAL LEGAL STUD. 21 (2008). See generally Eisenberg, *supra* note 5, at 22 tbl.2.

132. Illinois did pass tort reform in the period after the study. See *Lebron v. Gottlieb Mem'l Hosp.*, 930 N.E.2d 895, 899 (Ill. 2010) (noting damages cap passed in 2005 before overturning it).

133. Vidmar, *supra* note 3, at 340.

134. *Id.* at 348.

135. *Id.*

136. *Id.*

137. *Lebron*, 930 N.E.2d at 899.

of that state.¹³⁸ In their study, Bernard Black and his research group analyzed, as Vidmar had done, whether the claims of tort reform proponents regarding a "crisis" in litigation were true.¹³⁹ When they analyzed 15 years of insurance claim reports, those claims did not reflect the reality of litigation in Texas in the years preceding the 2003 reforms. Black et al. found that the number of claims, the number of large claims, the percentage of claims that were large, and total payout by insurance all remained stable in the decade or more prior to 2003.¹⁴⁰ In fact, paid claims per physician and the number of small claims had declined in the years preceding tort reform.¹⁴¹ They concluded, as Vidmar did, that "[t]his evidence suggests that no crisis involving malpractice claim outcomes occurred."¹⁴² So while insurance rates had increased, the source of the issue was not liability costs.¹⁴³

Parts of two other studies bear mentioning on the same issue. As part of their 2008 analysis of physician supply in the wake of Texas's tort reform of 2003, Charles Silver and his colleagues examined the physician supply issue in the years preceding passage of tort reform.¹⁴⁴ When they examined Texas's supply of direct care physicians as well as physicians per capita, the researchers found that Texas did not have a crisis in physician supply prior to 2003.¹⁴⁵ Instead, the supply of direct care physicians increased each year of their study, as did the physicians per capita.¹⁴⁶ Instead, they extrapolate that the number of physicians in Texas actually is lower after tort reform than it would have been without it.¹⁴⁷

Finally, as part of their 2004 survey of plaintiffs' attorneys in Texas, Stephen Daniels and Joanne Martin made an interesting observation about non-legal changes to the tort landscape.¹⁴⁸ While they examined the issue of post-tort reform access to attorneys for potential plaintiffs, they also analyzed the attorneys' perceptions of marketing and advertising on their work.¹⁴⁹ When asked about the effect of public relations campaigns on their work, a supermajority of plaintiffs'

138. Medical Malpractice and Tort Reform Act of 2003, H.B. 4, 78th Leg., Reg. Sess. (Tex. 2003). See generally Avraham, *supra* note 40.

139. Black et al., *supra* note 3, at 208 (noting proponents of tort reform adhere to the theory that "[m]ed mal liability is the disease, insurance rate spikes are the symptoms").

140. *Id.* at 209–10.

141. *Id.*

142. *Id.* at 210.

143. *Id.* at 255.

144. Silver et al., *supra* note 131, at 25.

145. *Id.*

146. *Id.* at 26 fig.1, 27 fig.3.

147. *Id.* at 27 (highlighting that the authors reject any causative claim here, however).

148. Daniels & Martin, *supra* note 69, at 1225.

149. *Id.* at 1241.

attorneys (91 percent) stated these campaigns had a negative impact on their practices.¹⁵⁰ The authors reported that “Texas plaintiffs’ lawyers fervently believe these campaigns have ‘poisoned’ the jury pool[.]”¹⁵¹ Daniels and Martin believe the effect of public relations campaigns is to harden insurance companies’ bargaining positions, making plaintiffs’ work more risky and more expensive.¹⁵² As a result, they believe that traditional legislative tort reform may be unnecessary to change the tort environment in Texas, as aggressive public relations soured market conditions for plaintiffs’ attorneys even before statutory change.¹⁵³

Collectively, the works of Vidmar, Black et al., and Daniels and Martin all suggest that, in retrospect, statutory tort reform may not have been necessary in the first instance.

III. OUR ANALYSIS ON THE EFFECT OF MEDICAL MALPRACTICE REFORM ON THE FILING OF CASES IN STATE COURT

In his comprehensive analysis of the empirical effects of tort reform, Eisenberg noted the difficulty in obtaining data necessary to analyze tort reform.¹⁵⁴ We believe that this data difficulty has made filing rate analysis the most underdeveloped area of analysis for tort reform research. We therefore intended to focus our study on that underdeveloped area of the literature—analysis of the effect of tort reform on filing rates—by researching the effect of tort reform on case filings in court.

Because tort reform contains so many separate aspects, discussed *supra* Section II.A,¹⁵⁵ we decided to limit our analysis to one aspect of tort reform: damages caps in medical malpractice cases. Two primary reasons for this choice exist: First, medical malpractice caps are one of the most common forms of tort reform, and have been in effect at one time or another in at least 30 states.¹⁵⁶ That would allow us to have ample test and control states for our analysis. Second, we found that by doing a preliminary assessment of the sets of data we did have access to, we could overcome the difficulties Eisenberg mentioned in data collection.¹⁵⁷

150. *Id.* at 1241 tbl.3.

151. *Id.* at 1242.

152. *Id.* at 1243.

153. Daniels & Martin, *supra* note 69, at 1262.

154. Eisenberg, *supra* note 5, at 15.

155. See *supra* text accompanying notes 40–46.

156. See ADVOCACY RES. CTR., AM. MED. ASS’N, CAPS ON DAMAGES (2011), available at <http://bit.ly/MtUvai>. See generally Avraham, *supra* note 40.

157. Eisenberg, *supra* note 5, at 15.

When we assembled the dataset and ran our analysis, we made a clear finding: changes to the available damages in medical malpractice have a clear effect on the filing rate of those cases.¹⁵⁸ We can see clearly that when a state adopts a damages cap, the number of filings will go down by 13 percent to 23 percent.¹⁵⁹ We then confirm the effect by discovering that when a state’s damages cap is nullified, the filings rate returns to the baseline by a corresponding increase of about 29 percent in filings.¹⁶⁰

A. Methodology

Our analysis builds upon the logistic-fixed effect approach we first applied in our papers establishing that the *Daubert*¹⁶¹ standard for scientific evidence is stricter than the *Frye*¹⁶² standard.¹⁶³ To perform the analysis described in this Article, we began by identifying a metric, grounded in the filing rate data, to measure the effect of medical malpractice caps: the relative proportion of medical malpractice torts filed to all torts filed in a given year.¹⁶⁴ We then created a database of over 2.5 million actual filings¹⁶⁵ spread over 15 states¹⁶⁶ and a 14-year

158. For the methodology of reaching this conclusion, see *infra* Parts III.A.1–4.

159. See *infra* Part III.B.

160. See *infra* Part III.B.

161. *Daubert v. Merrell Dow Pharm., Inc.*, 509 U.S. 579, 592–93 (1993).

162. *Frye v. United States*, 293 F. 1013, 1014 (D.C. Cir. 1923).

163. See Andrew Jurs & Scott DeVito, *Et Tu, Plaintiffs? An Empirical Analysis of Daubert’s Effect on Plaintiffs, and Why Gatekeeping Standards Matter (a Lot)*, 66 ARK. L. REV. (forthcoming 2014) (discussing analysis of nearly 3.5 million cases showing that plaintiffs’ attorneys believe *Daubert* to be a stricter standard); Andrew Jurs & Scott DeVito, *The Stricter Standard: An Empirical Assessment of Daubert’s Effect on Civil Defendants*, 62 CATH. U. L. REV. 675, 680 (2013) [hereinafter Jurs & DeVito, *The Stricter Standard*] (discussing analysis of over 4 million cases showing that defense attorneys believe *Daubert* to be a stricter standard).

164. We chose this metric for four reasons. First, it provides ample quantifiable data that limit researcher subjectivity. Second, because filings represent the onset of actual legal proceedings, it provides us with a representative sample of the number of actual disputes that is not distorted by settlements with sealed records. Third, it avoids selection bias that can arise when the researcher uses analysis of published cases or accepted appeals. Finally, it is superior to surveys in that it avoids the problem of inaccurate recall. See, e.g., Edward K. Cheng & Albert H. Yoon, *Does Frye or Daubert Matter? A Study of Scientific Admissibility Standards*, 91 VA. L. REV. 471, 483 (2005) (discussing the benefits of the removal rate metric).

165. There were a total of 255,791 tort case filings for the relevant periods combined in Alabama, Minnesota, and Washington. See Scott DeVito, Stata Log File for State Analysis (July 31, 2013) (on file with author). There were an additional 2,313,348 tort case filings from the ICPSR data. See Scott DeVito, Stata Log File for ICPSR Analysis (July 31, 2013) (on file with author). See *infra* Part III.A.1 for a discussion of the databases from which these data were pulled, and see *infra* Parts III.A.2–3, for a discussion of how these data were drawn from those databases.

span¹⁶⁷ that resulted in a real-world, case-by-case expression of the relative proportion of medical malpractice torts to all torts. We used that dataset to perform a series of logistic fixed effects analyses to identify any correlation between either (1) adoption of a medical malpractice cap and the filing of medical malpractice cases or (2) elimination of a medical malpractice cap and the filing of medical malpractice cases.¹⁶⁸

Using this metric, dataset, and a fixed effects statistical analysis, we determined that adoption of a medical malpractice cap *decreases* the likelihood that a medical malpractice tort case will be filed, while elimination of a medical malpractice cap *increases* the likelihood that a medical malpractice tort case will be filed.¹⁶⁹ These relationships establish for the first time, in a statistically significant manner,¹⁷⁰ that medical malpractice caps are barriers to tort victims filing in state court.

1. The Source Data

We performed our analysis using a database containing entries corresponding to over 2.5 million actual tort filings.¹⁷¹ Each entry had three fields: year, state, and a binary field indicating whether a particular entry corresponded to a medical malpractice tort or to some other kind of

166. We relied upon filings from Alabama, Arizona, Colorado, Florida, Minnesota, Mississippi, Missouri, Nevada, New Hampshire, New Jersey, New York, North Dakota, Oregon, Washington, and Wyoming. See Scott DeVito, Stata Log File for State Dataset Creation (July 31, 2013) (on file with author); Scott DeVito, Stata Log File for ICPSR Dataset Creation (July 31, 2013) (on file with author); see also *infra* Part III.A.3 (discussing the creation of the three Study datasets used in the fixed effects analysis).

167. Our data analyzed tort filings during the years 1992–1998 and 2000–2006. See Stata Log File for State Dataset Creation, *supra* note 166; Stata Log File for ICPSR Dataset Creation, *supra* note 166; see also *infra* Part III.A.3 (discussing the creation of the three Study datasets used in the fixed effects analysis).

168. See *infra* Parts III.A.4.b, III.B.

169. See *infra* Part III.B.

170. All measures of statistical significance discussed in this Article relate to the *p*-value of a statistical hypothesis. We will consider a result to be statistically significant if its corresponding *p*-value is less than or equal to 0.05. This means that there is no more than a one in twenty chance (or 5% chance) that our result is due to chance. DAVID A. HENSHER, JOHN M. ROSE & WILLIAM H. GREENE, *APPLIED CHOICE ANALYSIS: A PRIMER* 46–47 (2005). Using a *p*-value of 0.05 or less as a basis for statistical significance is consistent with general practice. See, e.g., *id.*; SCOTT E. MAXWELL & HAROLD D. DELANY, *DESIGNING EXPERIMENTS AND ANALYZING DATA: A MODEL COMPARISON PERSPECTIVE* 47 (2d ed. 2004).

171. There were a total of 255,791 tort case filings for the relevant periods combined in Alabama, Minnesota, and Washington. See Stata Log File for State Analysis, *supra* note 165. There were an additional 2,313,348 tort case filings from the ICPSR data. See Stata Log File for ICPSR Analysis, *supra* note 165.

tort.¹⁷² This database was created from four other databases. The largest was the publicly available court statistic database created by the National Center for State Courts (NCSC) and the Federal Judicial Center, available at the Interuniversity Consortium for Political and Social Research (ICPSR).¹⁷³ While the ICPSR database is comprehensive, there are gaps in its coverage. To fill some of these gaps, we requested data directly from the appropriate agencies in Alabama, Minnesota, and Washington for the period from 1991 to 2010 (the “State databases”).¹⁷⁴

2. Creating the Datasets

The ICPSR database contained summary information for all 50 states from 1985 to 2009¹⁷⁵ including, for each state, the total population of the state, the number of torts filed in the state, and the number of medical malpractice torts filed in state.¹⁷⁶ Because we use a logistic analysis, we could not keep the data in this format.¹⁷⁷ Instead, we needed

172. For example, a non-medical malpractice tort in 1996 North Dakota would have a data entry of <“North Dakota,” 1996, 0> while a medical malpractice tort in the same year and state would have a data entry of <“North Dakota,” 1996, 1>.

173. Our data were created using data drawn from a state database of court filings. The “State Database” was from the National Center for State Courts and contained the number of case filings in state courts as well as both adult and total population by state and year. NAT’L CTR. FOR STATE COURTS, STATE COURT STATISTIC SERIES: STUDY NO. 3990, STATE COURT STATISTICS, 2002: [UNITED STATES] (2004), *available at* <http://bit.ly/O1L3vU>; NAT’L CTR. FOR STATE COURTS, STATE COURT STATISTIC SERIES: STUDY NO. 9266, STATE COURT STATISTICS, 1985–2001: [UNITED STATES] ii (2005) [hereinafter STUDY NO. 9266], *available at* <http://bit.ly/1e2k2gQ> (noting that the “data collection provides comparable measures of state appellate and trial court caseloads by type of case for the 50 states, the District of Columbia, and Puerto Rico”); NAT’L CTR. FOR STATE COURTS, 09266-0024-CODEBOOK 1–46 (1997) (on file with authors) (listing variables and codes for trial court data from 1988–1992).

174. Our data were created using data from Alabama and Washington drawn from excel spreadsheets created and provided by each state’s Administrative Office of the Courts. E-mail from Scott DeVito, Assoc. Professor of Law, Fla. Coastal Sch. of Law, to Dean Hartzog, Assistant Legal Dir. & Pub. Info. Officer, Admin. Office of Courts (Jan. 21, 2011, 10:51 EST) (on file with author); E-mail from Andrew W. Jurs, Assistant Professor of Law, Fla. Coastal Sch. of Law, to Wash. State Ctr. for Court Research (Jan. 6, 2011, 10:01 EST) (on file with authors). The data from Minnesota were provided in the same format but were provided by the State Court Administrator’s Office. E-mail from Andrew W. Jurs, Assistant Professor of Law, Fla. Coastal Sch. of Law, to Debra Dailey (Jan. 5, 2011, 15:45 EST) (on file with authors).

175. STUDY NO. 9266, *supra* note 173, at ii (describing the “data collection [as] provid[ing] comparable measures of state appellate and trial court caseloads by type of case for the 50 states”).

176. See, e.g., NAT’L CTR. FOR STATE COURTS, STATE COURT STATISTICS, 1985–1995: [UNITED STATES] 6, 9–12 (1995) (on file with authors) (describing data in database).

177. In a statistical analysis there are two types of variables: independent and dependent. The dependent variables are the variables whose values explain the value of the independent variable. See DAMODAR N. GUJARATI & DAWN C. PORTER, ESSENTIALS OF ECONOMETRICS 8 (4th ed. 2010). Often the dependent variable is a continuous real

to take the summary data and create one entry for each tort filed. If the ICPSR data told us that in Year X, State A had 4,000 torts filed of which 300 were medical malpractice torts, then we created a dataset that contained 4,000 entries keyed to Year X and State A of which 3,700 were identified as “torts” and 300 were identified as “medical malpractice torts.”

Before we could create that dataset we needed to take into account population change because it could easily distort our results. Where population has increased during the relevant period, there may be a comparable increase in the raw number of torts and medical malpractice torts filed simply as a function of the increase in the number of people and not as a function of the rate of torts per person increasing. Similarly, where population has decreased over time, we may have a decrease in the raw number of torts and medical malpractice torts filed simply as a function of the decrease in number of people, not a decrease in the rate of filing.

For example, assume State A had 10,748 torts filed in 1985 of which 319 were identified as medical malpractice torts, but in 1993 State A had 12,940 torts filed of which 614 were medical malpractice torts. If we simply look at these numbers, then it appears that tort filings have increased by about 20 percent and medical malpractice filings have nearly doubled. But, if State A’s population significantly increased during this period of time, that view is misleading. Assume that in 1985 there were 3,187,000 people in State A, while in 1993 there were 3,936,000 people living in State A. During that eight-year period, State A saw a 23.5 percent growth in population. We would expect that the raw numbers of torts filed and medical malpractice torts filed would increase by a similar amount during this period as a direct result of the population growth. Only when the change in filings differs significantly from the population growth or decrease do we have good reason to think something other than population growth is in play.

One way to avoid the problem arising from population growth is to normalize the data relative to some base year.¹⁷⁸ In our case, we used 1985 as the base year. We then calculated a growth factor for each state and year by dividing the population in that state in 1985 by the

number like 3, 2.45, etc. But in some cases, the dependent variable is a category like smoker/non-smoker, male/female, true/false, 0/1, etc. These categorical variables (also called binary, nominal, or dichotomous variables) are better analyzed with a logistic analysis as compared to a linear ordinary least squares analysis. See DAMODAR GUJARATI, *ECONOMETRICS BY EXAMPLE* 142–44 (2011).

178. This is a fixed base method by which we use an index or base year to calculate the values of subsequent years. See, e.g., IRVING FISHER, *THE MAKING OF INDEX NUMBERS: A STUDY OF THEIR VARIETIES, TESTS, AND RELIABILITY* 19 (1st ed. 1922) (discussing the fixed base method).

population in that state in the later year. For State A in 1993, we have a factor of 0.81.¹⁷⁹ We then multiply the number of torts and the number medical malpractice torts filed in State A in 1993 by that factor to get a population-growth-normalized number of torts and medical malpractice torts. Doing so gives us a normalized value of 10,481 torts filed and 497 medical malpractice torts filed in 1993. Thus, by eliminating the effects of population growth, we discover that in State A, from 1985 to 1993, there was a 2.5 percent decrease in per capita tort filings while, simultaneously, there was a 35.8 percent increase in per capita medical malpractice tort filings.

Once we normalized the ICPSR summary data in this fashion we could use those normalized summary statistics to create a dataset that contained one entry for each tort filed where population change was no longer a potential confounding factor.¹⁸⁰ We then needed to supplement this dataset with the data from the State databases. The major difference between the ICPSR data and the State databases is that the ICPSR database is a state-by-state summary of filings in all 50 states while the State databases contain detailed information for each tort filed in a particular state. This difference matters because we needed the summary data to normalize the data to take into account population change. To do so, we created summary data from the State databases and then applied the same process we used for the ICPSR data to create a normalized dataset that took into account population change.¹⁸¹

3. The Study Populations

Using the normalized ICPSR and State datasets, we created four subpopulations to isolate the effects of adoption or elimination of medical malpractice caps. Our question concerns whether adoption or elimination of a medical malpractice cap has an effect on filings. Thus our treatment variables are: {adopts a medical malpractice cap, eliminates a medical malpractice cap}. To see the effect of the treatment variables we examined filing rates relative to those treatment variables in populations in which two other variables were held constant: {has a medical malpractice cap during the entire study period, does not have a medical malpractice cap during the entire study period}. This produced four subpopulations for analysis:

179. $0.81 = 3,187,000/3,936,000$.

180. See Stata Log File for ICPSR Dataset Creation, *supra* note 166.

181. See Stata Log File for State Dataset Creation, *supra* note 166. One principal difference in the normalization process was that we normalized the ICPSR data with 1985 as a base year, but normalized the Alabama, Minnesota, and Washington data with 1992 as a base year. See *id.*

	No cap for entire study period	Cap for entire study period
Adopts a cap during study period	1	2
Eliminates a cap during study period	3	4

TABLE 2. Subpopulations for analysis

Unfortunately, we were not able to acquire data that we could use to examine sub-population 4.

We then populated the three datasets (“Study datasets 1, 2, and 3”) using our ICPSR and State datasets with the relevant data.¹⁸² In each Study dataset we had “treatment” and “control” states. A “treatment” state is a state that either adopted or eliminated a medical malpractice cap at some point, and thus received the “treatment” during the study period, while a “control” state is a state that did not receive the treatment during the study period.¹⁸³ We restricted the study period to the three years before a treatment state adopted or eliminated a medical malpractice cap and the three years after adoption or elimination.¹⁸⁴ Because we presumed the year of adoption or elimination of a cap would be one in which confusion would reign, we eliminated that year from the study period. Thus if State A adopted a medical malpractice cap in 1999, then the study period would be 1996–1998 and 2000–2002.

a. Study Dataset 1: Florida, Mississippi, and North Dakota

Study dataset 1 compared states that adopted a medical malpractice cap during the study period to states that did not have a medical malpractice cap during the study period. Our datasets contained information sufficient to study two separate study periods. First, in 1995, North Dakota adopted a medical malpractice cap; thus, it is the 1995 treatment state. We compared North Dakota to six states, the controls, that did not have a medical malpractice cap from 1992–1998: Arizona,

182. See Stata Log File for State Dataset Creation, *supra* note 166; Stata Log File for ICPSR Dataset Creation, *supra* note 166.

183. See, e.g., MYOUNG-JAE LEE, MICRO-ECONOMETRICS FOR POLICY, PROGRAM, AND TREATMENT EFFECTS 1 (2005) (discussing treatment and control groups).

184. To limit problems of heterogeneity that could arise over time, we limited the study population to the three years before and after adoption or elimination of the cap. For a discussion of the concept of “heterogeneity,” see *infra* Part III.A.4.b.

Minnesota, Nevada, New York, Oregon, and Wyoming.¹⁸⁵ Second, in 2003, Florida and Mississippi adopted a medical malpractice cap, giving us two 2003-treatment states. There were five control states that did not have a medical malpractice cap in place from 2000–2006: Minnesota, New Hampshire, New Jersey, New York, and Oregon.¹⁸⁶ Not only is this temporally diverse, but, as Figure 2 demonstrates, it is also a fairly geographically diverse study population:

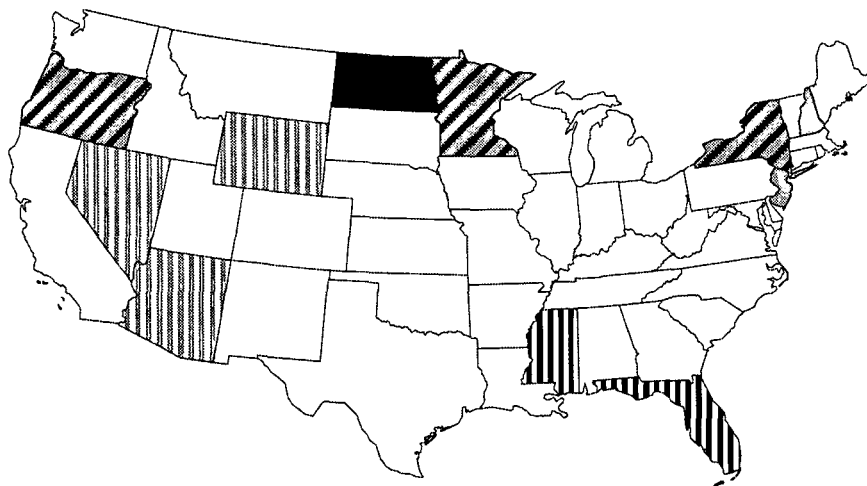


FIGURE 2. Geographic distribution of Study dataset 1¹⁸⁷

b. Study Dataset 2: Florida and Mississippi

Study dataset 2 compared states that adopted a medical malpractice cap during the study period to states that had a medical malpractice cap in place throughout the study period. For this dataset, we had data for one study period: 2000–2006. In 2003, Florida and Mississippi, the treatment states, adopted a medical malpractice cap. We compared the

185. See Avraham, *supra* note 40, at 17–19, 81–83, 95–98, 106–08, 110–12, 123–26, 154 (discussing adoption of tort reform in Arizona, Minnesota, Nevada, New York, North Dakota, Oregon, and Wyoming).

186. See *id.* at 36–42, 81–86, 99–103, 106–08, 123–26 (discussing adoption of tort reform in Florida, Minnesota, Mississippi, New Hampshire, New Jersey, New York, and Oregon).

187. States that are pure black represent the treatment state (North Dakota) for the 1995 series. States that are grey with white stripes represent states that are controls (Arizona, Nevada, and Wyoming) only for the 1995 series. States that are black with white stripes represent the treatment states (Florida and Mississippi) for the 2003 series. States that are pure grey represent controls (New Hampshire and New Jersey) only for the 2003 series. States that are grey with black stripes are controls (Minnesota, New York, and Oregon) for both the 1995 and the 2003 series.

treatment states to three states that had a medical malpractice cap in place during the period 2000–2006: Colorado, Missouri, and North Dakota, the control states.¹⁸⁸ While not as geographically widespread as the subpopulation for Study dataset 1, this subpopulation is still geographically broad:

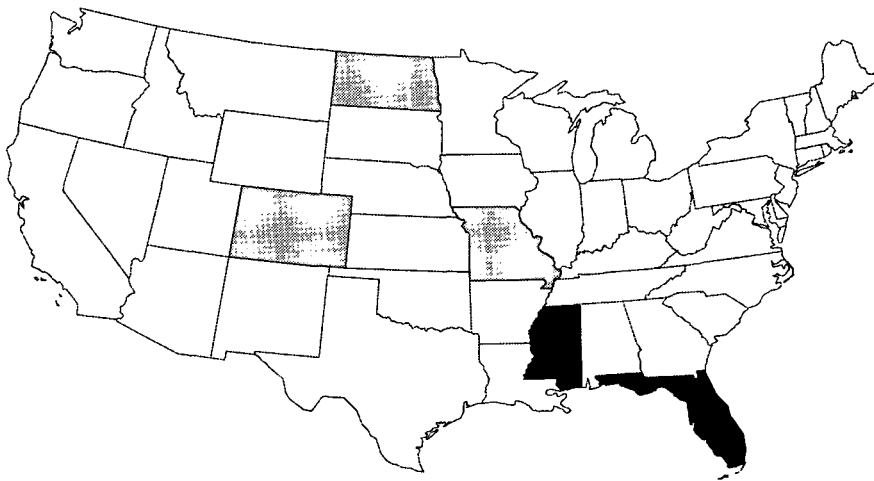


FIGURE 3. Geographic distribution of Study dataset 2¹⁸⁹

c. Study Dataset 3: Alabama

Study dataset 3 compared states that eliminated a medical malpractice cap during the study period to states that did not have a medical malpractice cap in place throughout the study period. This Study dataset proved to be the most difficult in terms of data. The ICPSR database either did not have data relating to states that dropped their medical malpractice coverage, or if it did, it did not have data for potential controls. As such, we were required to reach out to a number of states and purchase datasets directly from them.¹⁹⁰

In 1995, Alabama, the treatment state, adopted a medical malpractice cap. We compared it to the control states, Minnesota and

188. See Avraham, *supra* note 40, at 25–29, 36–42, 84–90, 110–12 (discussing adoption of tort reform in Colorado, Florida, Mississippi, Missouri, and North Dakota).

189. States that are pure black represent the treatment states (Florida and Mississippi). States that are pure grey (Colorado, Missouri, and North Dakota) are the controls.

190. See E-mail from Scott DeVito to Dean Hartzog, *supra* note 174; E-Mail from Andrew W. Jurs to Wash. State Ctr. for Court Research, *supra* note 174; E-Mail from Andrew W. Jurs to Debra Dailey, *supra* note 174.

Washington, neither of which had a medical malpractice cap from 1993–1998.¹⁹¹ This gives us the geographically wide, but sparse, coverage:

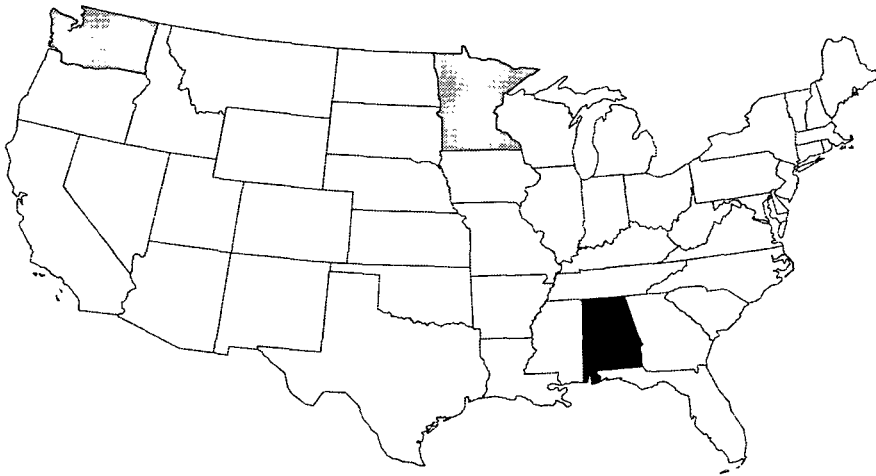


FIGURE 4. Geographic distribution of Study dataset 3¹⁹²

4. Theory and Statistical Background

The goal of our analysis was to determine the effect of medical malpractice caps on malpractice tort filings. A statistical analysis can tell us whether two events are correlated with each other, but that correlation does not provide a causal explanation outside of some theory that explains why one of the events causes the other.¹⁹³ As such, we turn to our expectations about the effect adoption and elimination of medical malpractice caps have on medical malpractice filings.

a. Medical Malpractice Caps Lower Both the Attorney’s and the Injured Party’s Expected Utility for Filing a Case

When a client comes to an attorney seeking representation, representation will only arise if both the client and the attorney are

191. See Avraham, *supra* note 40, at 9–12, 81–83, 145–46 (discussing adoption of tort reform in Alabama, Minnesota, and Washington).

192. States that are pure black represent the treatment states (Alabama). States that are pure grey (Minnesota and Washington) are the controls.

193. See, e.g., DAVID R. HEISE, CAUSAL ANALYSIS 152 (1975) (discussing how statistical analysis can lead to causal inference only in the context of a theory); 2 MAURICE G. KENDALL & ALAN STUART, THE ADVANCED THEORY OF STATISTICS 279 (1961) (“A statistical relationship, however strong and however suggestive, can never *establish* causal connection: our ideas of causation must come from outside statistics, ultimately from some theory or other.” (emphasis added)).

“satisfied” with the deal. Assuming, for ease of argument, that attorneys are purely rational beings and that they make all decisions on the basis of their economic self-interest, this means that an attorney will undertake to represent a client only if the attorney believes that the expected utility of taking on the case (the probability of winning times the expected fee) is above some threshold. In essence, the attorney must believe that there is a “good” chance that the attorney will gain some level of economic gain. For example, if we assume a contingency fee arrangement and that attorneys want to earn a \$100,000 fee, then Figure 5 shows us that, given the probability of success and the expected damages award, some cases would fail to receive representation:

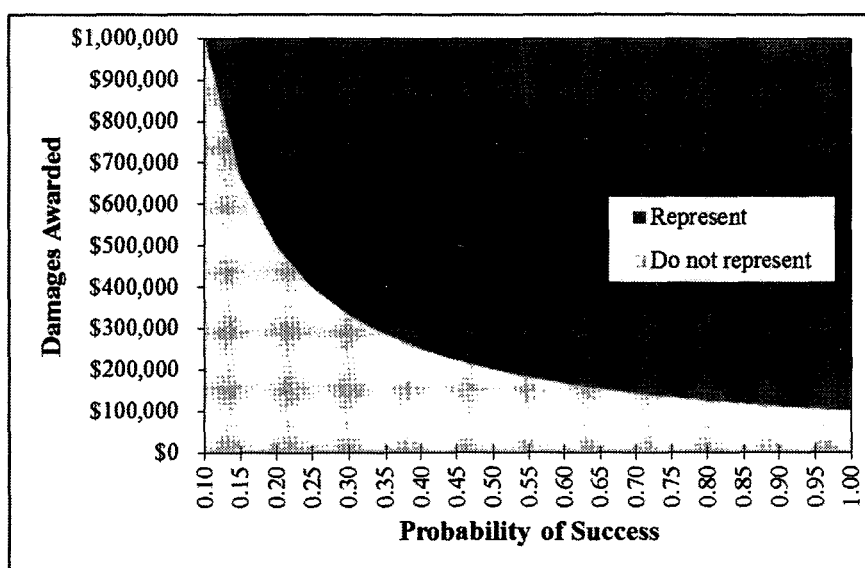


FIGURE 5. Representability given no cap present

When a state imposes a medical malpractice cap, it decreases the expected damages award for a number of cases by lowering the maximum award available. For example, as depicted in Figure 6, if our medical malpractice cap limited damages to \$500,000, this would mean that the expected utility for a number of cases would fall into the “do not represent” category when, before the cap, they would be cases for which one could find representation:

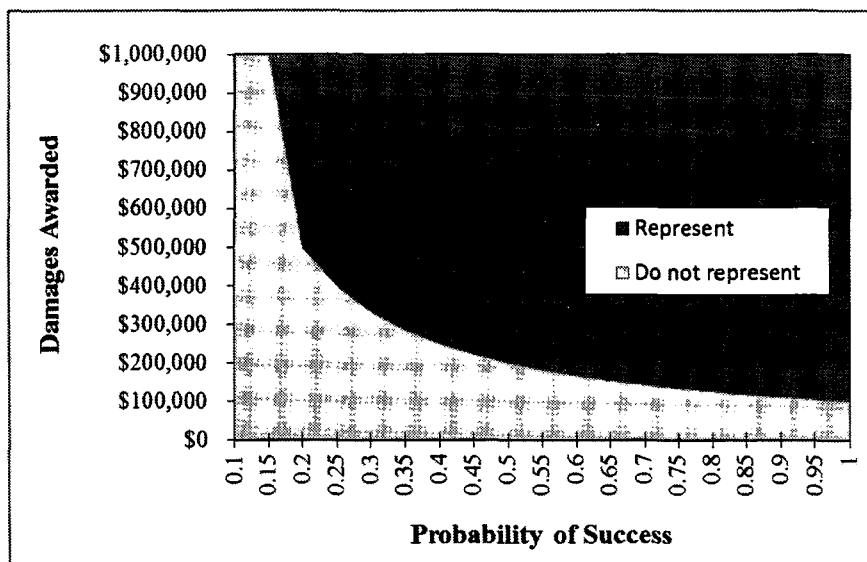


FIGURE 6. Representability with cap present

The results in Figure 6 might change if the attorney did not hold other factors constant. For example, the introduction of a cap lowers the expected utility, and thereby the "representability," of a case by decreasing the expected fee *only if* one holds one's fee constant. If the attorney raises the fee charged, then, at least some of those newly un-representable cases become representable again.

The injured party faces a double-effect from the introduction of a medical malpractice cap. First, the potential award is lowered. This will decrease the expected utility of suing, as calculated in terms of probability of success times expected award, and thereby decrease the incentive for the injured party to sue. Second, if the injured party is now in the un-representable category due to the introduction of the cap, he or she might be able to get representation if he or she allows the attorney to increase the fee charged. That, however, has the effect of lowering the expected utility of suing by *further* lowering the expected award.

Therefore, the introduction of a medical malpractice cap should lower the relative proportion of medical malpractice torts filed to all torts filed because the number of cases attorneys will be interested in undertaking should drop,¹⁹⁴ while, simultaneously, the number of cases injured persons are willing to undertake should decrease. For example, if State A adopted a medical malpractice cap in 1995, we should see a

194. As discussed *supra*, other studies have shown that this is precisely what happens. See *supra* text accompanying notes 66–77, 148–53.

decrease in the relative proportion of medical malpractice torts filed in the years after adoption:

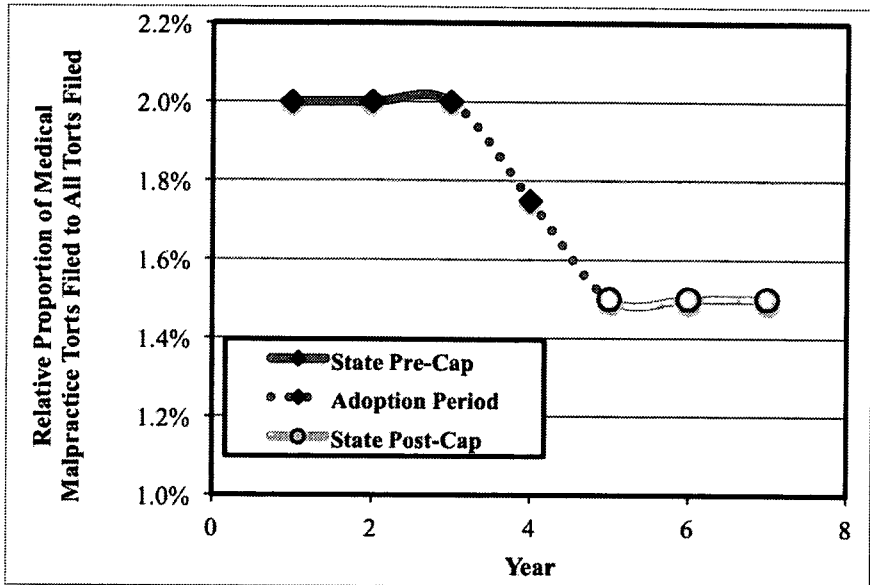


FIGURE 7. Theoretical effect of adoption of a cap

The removal of a medical malpractice cap should have the opposite effect. For attorneys, cases that did not have a sufficiently high expected utility will now be in the range for representation. Moreover, those cases may be taken on for lower fees because the expected award may be much higher. Similarly, it is more desirable for an injured person to sue because the expected return can be higher both directly, through increases to awards, and indirectly, through decreases in attorney fees. Thus, removing medical malpractice caps should raise the relative proportion of medical malpractice torts filed to all torts filed. For example, if State B eliminates a medical malpractice cap in 1995, we should see the relative proportion of medical malpractice cases increase in subsequent years:

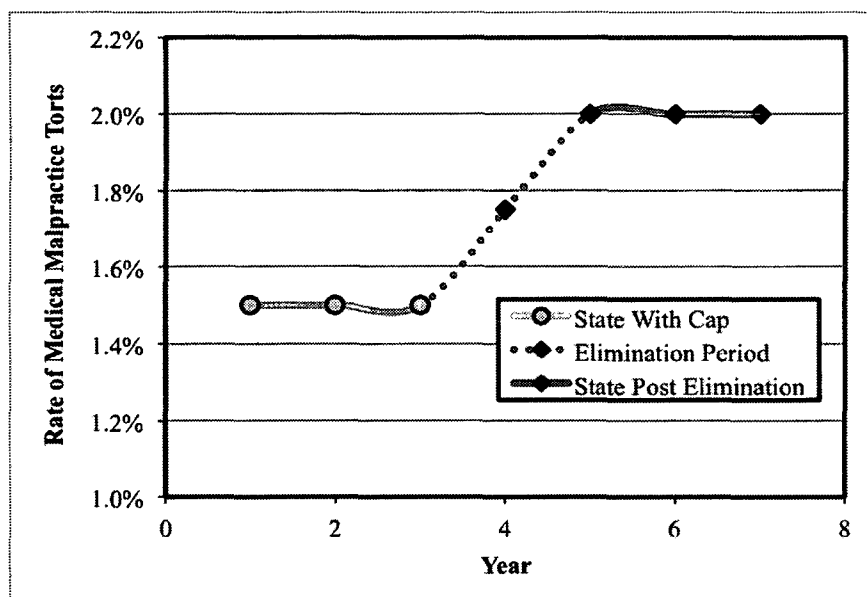


FIGURE 8. Theoretical effect of removing a cap

Thus, we begin our analysis with the theory that the introduction of a medical malpractice cap creates a bar to entry to the court system for litigants by lowering the expected utility of filing for both plaintiffs' attorneys and the potential plaintiffs. We should see lowered utility play out in two ways. First, if a state previously had no medical malpractice cap, imposition of one should either lower the relative proportion of medical malpractice torts filed to all torts filed, or, if the relative proportion is increasing, decrease the rate of growth. Second, if a state previously had a medical malpractice cap, then elimination of it should either raise the relative proportion or, if the relative proportion was decreasing, slow the rate of decrease. As noted below, this is precisely what we find.

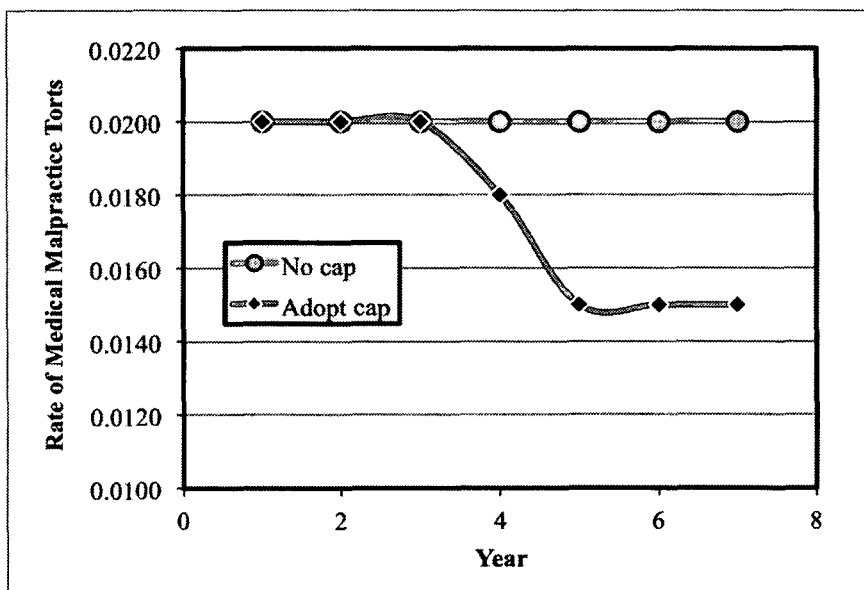
b. Fixed Effects Analysis

When the treatment and control populations differ with regard to some statistically relevant factor, the populations are called "heterogeneous."¹⁹⁵ Heterogeneity makes statistical analysis more complex because differences seen in the study variable (e.g., filing rates)

195. See, e.g., RONALD A. FISHER, *THE DESIGN OF EXPERIMENTS* 32–33 (8th ed. 1966) (discussing the problem of heterogeneity in the context of pairing and grouping); GUJARATI, *supra* note 177, at 5 (discussing the problem of heterogeneity); LEE, *supra* note 183, at 9–10 (discussing causal inference as compared to statistical association).

could be caused by differences in the treatment and control populations *other than* the presence or absence of the treatment (e.g., adoption of a medical malpractice cap).¹⁹⁶ Moreover, some of the differences in the populations may be entirely unknown.¹⁹⁷ Therefore, any effect identified could be caused by the treatment, the known differences in the populations, or the unknown differences in the population.

The "ideal" way to deal with heterogeneity is to run a randomized, controlled experiment.¹⁹⁸ In our case, we would identify a set of states, States A–N, that do not have a medical malpractice cap. We would then record the values of all of the relevant variables relating to those States at time (*t*), randomly assign each state to either treatment (adopts a medical malpractice cap) or control (does not adopt a medical malpractice cap), and record the filing rate each subsequent year until time *t'*. Under our theory, we would expect this randomized controlled experiment to produce an effect something like that in Figure 9:



**FIGURE 9. Theoretical effect of adoption of a cap
in a controlled, repeatable experiment**

196. See, e.g., FISHER, *supra* note 195, at 32–33; GUJARATI, *supra* note 177, at 5; LEE, *supra* note 183, at 9–10 (discussing causal inference as compared to statistical association).

197. See, e.g., LEE, *supra* note 183, at 18 (discussing the impossibility in any experiment of making the various test populations identical in all relevant respects).

198. See, e.g., CHRISTOPHER H. ACHEN, *THE STATISTICAL ANALYSIS OF QUASI-EXPERIMENTS 1–2* (1986) (discussing randomized, controlled experiments).

For many studies, including ours, a controlled, randomized experiment is simply not possible. In such circumstances, we turn to “quasi-experimentation.”¹⁹⁹ A quasi-experimental study is an empirical study that differs from traditional experimental design or randomized controlled studies in that it specifically lacks the element of random assignment to treatment or control.²⁰⁰

Figures 7 and 8 represent a kind of quasi-experimental statistical analysis called a “before-after analysis.”²⁰¹ In a before-after analysis, we measure the effect a treatment has on a population by comparing the tested variable before and after the treatment is given.²⁰² In essence, we compare the rate of medical malpractice filings in North Dakota in the period before North Dakota adopts a medical malpractice cap (1992–1994) to the period after North Dakota adopts a cap (1996–1998). The before-after analysis measures change over time *within the same group*, presuming that any change in the tested variable is due *solely* to the treatment.²⁰³ The problem with a before-after analysis is that there might be some unknown covariate that changes over time within the population and alters the measured variable.²⁰⁴ As a result, it is possible that a measurable effect might be caused by something other than the treatment.²⁰⁵

A second kind of quasi-experimentation called “matching” works by finding a state that has exactly the same relevant features as State A other than the treatment variable, and then comparing how the rate of medical malpractice filing differs between those two states.²⁰⁶ In matching we compare two populations (e.g., State A and State B) over the same period during which one population receives the treatment and the other does not.²⁰⁷ Importantly, we must pick two populations that are comparable²⁰⁸ and are presumed to have the same covariate values.²⁰⁹ Unfortunately, it is extremely hard to identify *all* of the relevant factors and find a comparable state that has exactly the same factors, except the

199. *See id.* at 2–5.

200. *See id.*

201. *See LEE, supra* note 183, at 64–65.

202. *See id.*

203. *See id.* at 65.

204. *See id.* at 65, 79, 99.

205. *See id.*

206. *See LEE, supra* note 183, at 79, 99.

207. *See id.* at 79, 99.

208. James J. Heckman, Hidehiko Ichimura, & Petra Todd, *Matching as an Econometric Evaluation Estimator*, 65 REV. ECON. STUD. 261, 261 (1998). Two populations are “comparable” if both populations “would have experienced the same outcomes . . . had they participated in the programme[.]” *Id.* at 262. A central difficulty with matching is ensuring that the two groups are comparable. *Id.*

209. *See LEE, supra* note 183, at 79.

treatment factor, as the treatment state during the study period.²¹⁰ There is always a risk that some unknown variable could explain the result.²¹¹

We can avoid many of these problems by combining before-after analysis with a matching analysis.²¹² In essence, if we pick two comparable populations where the only difference between the two is that one receives the treatment and the other does not, then any difference in outcome for the study variable (e.g., rate of malpractice torts filed) between the two states must be a result of the treatment because both states have all other variables in common.²¹³

As we noted, the matching analysis has problems because we cannot be sure that we have found two comparable populations. In our case, we cannot be sure that our treatment and control states in the years we are studying them are comparable to each other. To deal with this problem, we can engage in a fixed effects regression analysis.²¹⁴ In an ordinary linear regression analysis we would attempt to analyze the relationship between a dependent variable—in our case, rate of medical malpractice torts—and a set of independent variables—in our case, whether the state has adopted a cap, the year, and a state identifier.²¹⁵ We could do so by performing a regression analysis on:

$$\text{Rate}_{i,t} = \alpha_1 + \beta_{i,t}T_{i,t} + \gamma_1S_1 + \gamma_2S_2 + \dots + \gamma_nS_n + \delta_1Y_{1,t} + \delta_2Y_{2,t} + \dots + \delta_6Y_{6,t} + \mu_{i,t}$$

where the subscript i identifies the state (so $i = 1$ for state A, 2 for state B, and so on);
 the subscript t identifies the year (so $t = 1$ for year 1, 2 for year 2, etc.);
 $\text{Rate}_{i,t}$ is the medical malpractice torts filing rate at time t in state i ;
 $T_{i,t}$ is a set of binary variables such that that is set to 1 if the i^{th} state at time t had a medical malpractice tort and set to 0 otherwise;

210. See *id.* at 88–90 (discussing evaluating the success of matching); see also FISHER, *supra* note 195, at 32 (discussing Darwin’s pairing comparisons).

211. See FISHER, *supra* note 195, at 32; LEE, *supra* note 183, at 88–90.

212. See LEE, *supra* note 183, at 65, 79, 99.

213. See *id.*

214. See *id.* at 79 (noting that difference-in-differences analyses “can deal with unobserved confounders to some extent”); see also Jurs & DeVito, *The Stricter Standard*, *supra* note 163, at 716–23 (discussing that “difference-in-differences” models are a special case of fixed effect analysis).

215. GUJARATI & PORTER, *supra* note 177, at 8 (discussing econometrics models).

S_i is a set of numeric variables that identify the i^{th} state;

Y_n is a set of numeric variables that identify each year;

α_1 is the y intercept;

$\beta_{i,t}$ is the regression coefficient for the medical malpractice tort binary variables for state i at time t ;

δ_i is the regression coefficient for year i ; and

$\mu_{i,t}$ is the standard error term.

EQUATION 1

The problem with using an ordinary linear analysis is twofold. First, the rate of filing is not a sufficiently sensitive measure to identify the effect of adoption or elimination of a medical malpractice cap. This requires us to adopt a one-entry-per-tort analysis in which the dependent variable is a binary variable that is set to 1 if the tort was a medical malpractice tort, and zero otherwise. Logistic regression, not linear regression, is generally preferred when the independent variable is categorical or binary.²¹⁶ Second, because we are not certain whether our treatment and control states are comparable, we cannot be sure we have dealt with all of the heterogeneity simply by combining matching with before-after analysis. In a further effort to avoid the consequences of unknown covariates, we used a logistic fixed effects analysis with dummy variables to determine if there was any correlation between filing of a medical malpractice tort and adoption or elimination of a medical malpractice cap. The advantage of a fixed effects analysis with dummy variables is that it enables us to isolate more of the effects of unknown variables.²¹⁷

A fixed effects analysis²¹⁸ is a regression analysis that is performed on a regression formula that contains a set of dummy variables²¹⁹ that are

216. See, e.g., ALAN AGRESTI & CHRISTINE FRANKLIN, STATISTICS 610 (2007); GUJARATI & PORTER, *supra* note 177, at 387–89.

217. See LEE, *supra* note 183, at 79; see also Jurs & DeVito, *The Stricter Standard*, *supra* note 163, at 720–23.

218. For a more detailed description of fixed effects analysis in general, see Jurs & DeVito, *The Stricter Standard*, *supra* note 163, at 716–23.

219. Dummy variables are variables that have a value of 1 if a condition is met and a value of 0 otherwise. GUJARATI, *supra* note 177, at 47. In our case, we create one dummy variable for each year and for each state. So we would have *Dummy_Arizona*, *Dummy_Florida*, etc. and *Dummy_1985*, *Dummy_1986*, etc. A dummy variable is set to 1 if it is true of that entry but set to 0 otherwise.

designed to capture individual-specific unknowns.²²⁰ In our case, we restructured the regression formula to contain dummy variables to account for state- and year-specific unknowns.²²¹ Combined with our logistic approach, this produces the following formula:

$$\text{TypeOfTort}_{i,t,n} = \alpha_1 + \beta_1 S_{1,i} + \beta_2 S_{2,i} + \gamma_1 Y_{1,t} + \gamma_2 Y_{2,t} + \dots + \gamma_6 Y_{6,t} + \mu_{i,t}$$

where the subscript i identifies the state (so $i = 1$ when the state is Alaska, 2 if the state is Arizona, etc.); the subscript t identifies the year (so $t = 1$ for 1990, 2 for 1992, etc.); the subscript n identifies this as the n^{th} tort filed in state i and year t ;
TypeOfTort _{i,t,n} is a 1 if the n^{th} tort in year t and state i was a medical malpractice tort and 0 otherwise;
 $S_{m,i}$ is a set of binary variables such that $S_{1,i}$ is set to 1 when i is 1 (the state is Alaska) and to 0 otherwise, $S_{2,i}$ is set to 1 when i is 2 (the state is Arizona) and to 0 otherwise, etc.;
 $Y_{r,t}$ is a set of binary variables such that $Y_{1,t}$ is set to 1 when $t = 1$ (the year is 1985) and to 0 otherwise, $Y_{2,t}$ is set to 1 when $t = 2$ (the year is 1986) and to 0 otherwise, etc.;
 α_1 is the y intercept;
 β_i is the regression coefficient for state i ;
 γ_i is the regression coefficient for year i ; and
 $\mu_{i,t}$ is the standard error term.

EQUATION 2

220. The “fixed” in fixed effects model corresponds to the idea that the intercept for each individual (in our case, states and years are individuals) is time invariant. We then use the intercept for the first individual as the benchmark intercept. The dummy variables “will show by how much the intercept coefficient of the individual that is assigned a dummy variable differs from the benchmark category.” *Id.* at 283.

221. When we perform the regression analysis using the formula, we must take care to avoid a problem called the “dummy variable trap.” *See id.* In our case, because we have one dummy variable for each state and one for each year, we will encounter perfect collinearity between (at least) one state dummy variable and one year variable and the rate of removal intercept. *See id.* at 48, 283. To avoid this problem, we generally drop one state dummy variable and one year dummy variable when we perform the regression analysis. *See id.*

At this point, the heterogeneity in the system should be captured and isolated by the model.

B. *The Results*

Our first study population compared states that adopted a medical malpractice cap during the study period to states that did not have a medical malpractice cap in place during the study period. We had two study periods, 1992–1998 and 2000–2006, and two sets of treatment states. For the first period, North Dakota was compared to six control states: Arizona, Minnesota, Nevada, New York, Oregon, and Wyoming. For the second period, Florida and Mississippi were compared to five control states: Minnesota, New Hampshire, New Jersey, New York, and Oregon.

In this study population, we would expect that the probability that a tort filed is a medical malpractice tort to be less when there is cap in place, as opposed to when there is not. The logistic fixed effects analysis of this study population and period produced statistically significant effects that support this hypothesis. We found that adoption of a medical malpractice cap decreased the probability that a tort would be a medical malpractice tort by 23 percent.²²²

Adopted medical malpractice cap	Probability that tort will be a medical malpractice tort
Yes	0.0373648
No	0.0487044

TABLE 3. Study population 1²²³

Study dataset 2 compared states that adopted a medical malpractice cap during the study period to states that had a medical malpractice cap in place throughout the study period. For this dataset, we had data for one study period, 2003, when Florida and Mississippi, the treatment states, adopted a medical malpractice cap. We compared the treatment states to Colorado, Missouri, and North Dakota, the control states, all of which had a medical malpractice cap in place throughout the study period.

In this study population, we would expect the probability that a tort filed is a medical malpractice tort to be less when there is cap in place as

222. See Stata Log File for ICPSR Analysis, *supra* note 165.
223. The correlation coefficient for the cap adoption variable had a *p*-value of less than 0.0005, meeting the *p*-value of less than or equal to 0.05 test for statistical significance.

opposed to when there is not. At the same time, we would expect the change to be smaller relative to study population 1, because in study population 1 the controls had no caps, which means they should have higher base rates of filing, while in study population 2, the controls have caps which means they should have lower (compared to the controls in study population 1) base rates of filing. The logistic fixed effects analysis of this study population and period produced statistically significant effects that support this hypothesis. We found that adoption of a medical malpractice cap decreased the probability that a tort would be a medical malpractice tort by 13 percent (which is considerably less than the 23 percent decrease for study population 1):²²⁴

Adopted medical malpractice cap	Probability that tort will be a medical malpractice tort
Yes	0.0589297
No	0.0675025

TABLE 4. Study population 2²²⁵

Study dataset 3 was restricted to the period 1993–1998 and compared Alabama, which eliminated a medical malpractice cap in 1995, to Minnesota and Washington, neither of which had a medical malpractice cap during the study period. In this study population, we would expect the probability that a tort filed is a medical malpractice tort to increase when the cap is removed. That is precisely what we found. We found that not having a medical malpractice cap increased the probability that a tort would be a medical malpractice tort by 29 percent:²²⁶

Eliminated medical malpractice cap	Probability that tort will be a medical malpractice tort
Yes	0.028013
No	0.0199264

TABLE 5. Study population 1²²⁷

224. See Stata Log File for ICPSR Analysis, *supra* note 165.

225. The correlation coefficient for the cap adoption variable had a *p*-value of less than 0.0005, meeting the *p*-value of less than or equal to 0.05 test for statistical significance.

226. See Stata Log File for State Analysis, *supra* note 165.

227. The correlation coefficient for the cap adoption variable had a *p*-value of less than 0.0005, meeting the *p*-value of less than or equal to 0.05 test for statistical significance.

IV. DISCUSSION

By examining court data from 15 states between 1992 and 2006,²²⁸ we found that when a state adopts a damages cap in medical malpractice cases the number of medical malpractice torts drops by over 20 percent, a statistically significant change compared to non-cap states.²²⁹ We can confirm the modification of the filing rate in reaction to tort reform by examining the opposite situation; using the nullification of Alabama’s damages cap in 1995, we have also found that the percentage of medical malpractice torts significantly increases when the downward pressure of the cap evaporates.²³⁰ If we assume a world where medical malpractice filings are essentially static, then, the effect of medical malpractice caps can be depicted graphically as follows:

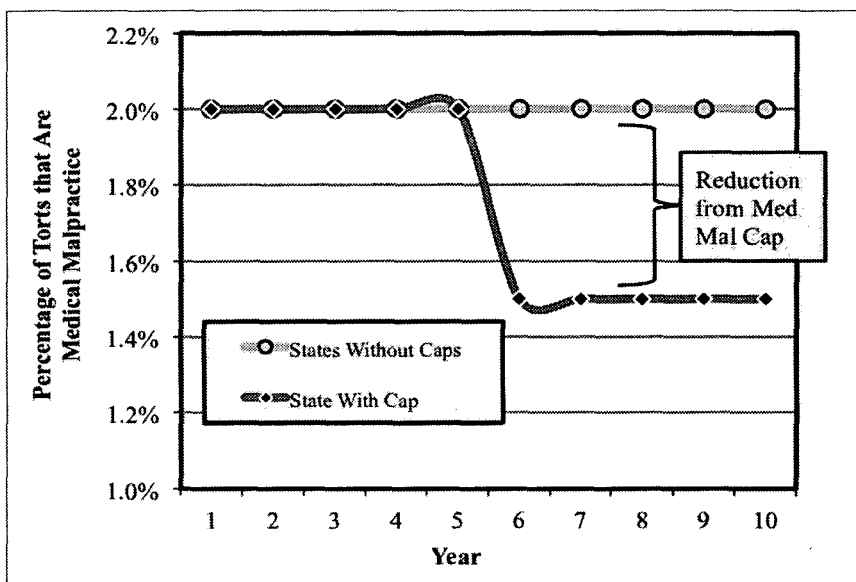


FIGURE 10. Effect of med mal caps, assuming a static baseline

While that finding is significant and interesting, it assumes that—all things being equal—the background of case filings would remain a flat line with neither growth nor decline. Yet, recent research has challenged

228. Alabama, Arizona, Colorado, Florida, Minnesota, Mississippi, Missouri, Nevada, New Hampshire, New Jersey, New York, North Dakota, Oregon, Washington, and Wyoming. See Stata Log File for ICPSR Dataset Creation, *supra* note 166; Stata Log File for State Dataset Creation, *supra* note 166.

229. See *supra* Part III.B (finding of a 13% to 23% reduction in med mal filings after cap adoption).

230. See *supra* Part III.B (finding a 29% increase in filings after cap nullification).

that basic assumption, and in so doing, significantly increased the importance of the finding we have made. In their 2013 study, Myungho Paik and his colleagues analyzed med mal filings in states with caps and those without such caps.²³¹ While the researchers found that med mal filings drop in states with tort reform, they also found that med mal filings drop 24 percent *even in states without damages caps*.²³² A recent series of studies from the NCSC supports the same conclusion. In their 2011 work for the Court Statistics Project of the NCSC, Cynthia Lee and Robert LaFountain analyzed med mal filings from 1999 to 2008, and found filings had decreased 15 percent.²³³ The analysis has been updated recently, and the filings decline has only grown: medical malpractice case filings dropped 23 percent in the decade from 2001 to 2010.²³⁴

Considering those studies along with our finding, then, we have discovered something quite serious. Because Paik et al. and Lee and LaFountain can show between a 15 percent and 24 percent drop in med mal filings in non-cap states in the 2000s, and because we can show a drop in filings for cap states that is statistically significant compared to non-cap states, our data reveal that the effect of tort reform is not as depicted in Figure 10, but instead actually looks like this:

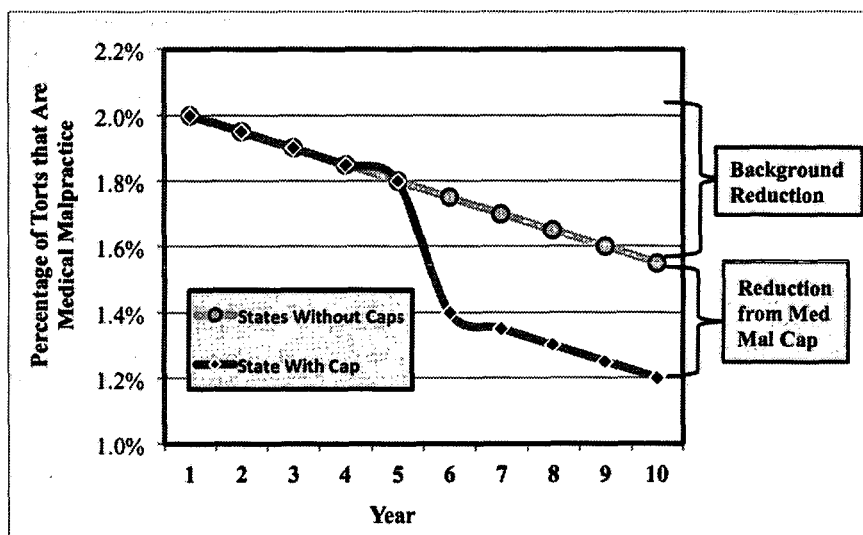


FIGURE 11. Effect of med mal caps,
actual declining baseline

231. Paik et al., *supra* note 11, at 612.

232. *Id.* at 625 tbl.2; *see also supra* text accompanying note 86 (indicating a 42.4% decline in cap states and a 23.8% decline in no cap states).

233. LEE & LAFOUNTAIN, *supra* note 86, at 3.

234. *Court Statistics Project*, *supra* note 86.

Instead of a single decline in filings attributable to the adoption of the damages cap, the filing rate for med mal cases instead is subject to a "doubling-down" effect by the impact of two separate declines: one attributable to adoption of a damages cap, and one occurring to all states as a background effect.

The finding and quantification of the "doubling-down" effect of tort reform is interesting by itself, but we also intend to examine the greater implication of this finding by examining the impact this finding should have on cost-benefit analyses of tort reform in the future. To do so, we will first examine the purported purposes behind tort reform to examine the benefits-side component, and then switch to the effects in light of this doubling effect. Afterward, we will distill these greater findings into a final policy prescription.

A. Benefits Analysis: At Best Mixed Support for Claims by Proponents

Whether examining the effects of tort reform on the legal system or on the larger world, the empirical evidence supporting tort reforms seems mixed at best, which leads to the conclusion that the benefits of tort reform have been significantly oversold. Tort reform was enacted with the goals of addressing health care professionals' dissatisfaction with rising insurance rates and constraining "jackpot justice" in personal injury compensation.²³⁵ However, when one examines the studies on filing rates prior to tort reform or in places without it, a different picture emerges. In their 2005 study, Bernard Black and his colleagues examined rates of insurance claims in Texas prior to adoption of tort reform in 2003.²³⁶ They found that, instead of increasing, both the number of claims per physician and number of small claims had *declined* in the years leading to tort reform, while large claims remained unchanged.²³⁷ Texas does not provide the only example, however. After examining the tort filings in Illinois prior to tort reform in Illinois, Neil Vidmar unequivocally stated there was no evidence of an increase in filings in med mal lawsuits.²³⁸ He concluded: "juries are not to blame for the problems involving the increases in doctor's liability premiums. It is time to look for other causes of the ailment."²³⁹ The recent data from the NCSC support both studies, finding that rates of med mal filings have declined over 20 percent between 2001 and 2010, regardless

235. See WEILER, *supra* note 1, at 12; TORT POLICY STUDY GROUP, *supra* note 31, at 5; *Republican Platform 2000*, *supra* note 1.

236. Black et al., *supra* note 3, at 208.

237. *Id.* at 209–10.

238. Vidmar, *supra* note 3, at 348.

239. *Id.*

of tort reform. Empirical research data do not support one major underlying precondition—a crisis of rapidly rising filing rates—necessitating tort reform.

Similarly, if one examines damages paid instead of filings made, the empirical findings do not support the “jackpot justice” theory of tort reform. Clearly, damages caps reduce the payout to plaintiffs suing health care providers, and many studies support those findings.²⁴⁰ Yet, when one digs into the data deeper, the effect is not as clear. One analysis—controlling for the effect of severity of injury—found damages caps had no significant effect on compensatory damages.²⁴¹ In addition to these studies, two additional points blunt the benefit of the cap. First, Daniels and Martin have found that plaintiffs’ attorneys believe that a public relations campaign can be just as, if not more, effective in reducing payouts to plaintiffs than direct tort reform legislation.²⁴² Second, juries are increasingly skeptical of plaintiffs in medical malpractice cases. In his 2009 study, Thomas Cohen found that “win rates” for plaintiffs in med mal cases were less than 23 percent, while all plaintiffs had “win rates” of over 50 percent.²⁴³ By combining the findings of Daniels and Martin and Cohen, we conclude that while damages in court do show significant reductions, the effect of tort reform may not be the entire story, and instead greater forces may be in play. At best, the claim of proponents that tort reform is necessary to reduce large pro-plaintiff verdicts lacks substantial support and is subject to significant limitations.

In addition to these studies on the effect of tort reform on the legal system, there are many studies on the proponents’ claims of beneficial effects outside the legal system, such as ending defensive medicine or keeping health care providers in a specific area. Once examined in detail, however, the empirical data do not support these asserted benefits. In a series of studies since 2006, researchers have found that tort reform does not reduce medical costs as anticipated by its proponents. A 2006 study by the non-partisan CBO concluded that tort reform did not demonstrate any effect on Medicare expenditures.²⁴⁴ A 2009 study by Frank Sloan and John Shadle came to the same conclusion: direct

240. PACE ET AL., *supra* note 52, at 35–41; Hyman et al., *supra* note 62, at 399; Studdert et al., *supra* note 50, at 58; *see also supra* text accompanying notes 50–65.

241. Sharkey, *supra* note 58, at 445.

242. Daniels & Martin, *supra* note 69, at 1242–43, 1262.

243. THOMAS H. COHEN, U.S. DEP’T OF JUSTICE, TORT BENCH AND JURY TRIALS IN STATE COURTS, 2005, at 4 tbl.4 (2009), *available at* <http://1.usa.gov/1bJn8Mt>.

244. CONG. BUDGET OFFICE, *supra* note 117, at 35; *see also supra* text accompanying notes 117–18.

reforms, such as damages caps, do not reduce Medicare spending.²⁴⁵ Finally, Myungho Paik and his colleagues tested the same hypothesis by examining the effect of tort reform in high-risk and low-risk counties in Texas, thinking that if tort reform lowered “defensive medicine” the costs in high-risk counties should go down.²⁴⁶ Instead of finding this pattern, however, they concluded that spending in high-risk counties either remained stable or increased; therefore, tort reform did not reduce Medicare spending as suggested by proponents.²⁴⁷

A second benefit of tort reform suggested by proponents involves the claim that physicians will move to areas of lower liability exposure, so states without tort reform may have a doctor supply problem. Since 2005, four separate studies examined physician supply and tort reform and concluded that there is no connection between the two.²⁴⁸ Of those studies that have found a limited effect, the effect of tort reform as a selection criterion by physicians pales in comparison to other factors such as climate, HMO penetration, or availability of residency programs.²⁴⁹ At best, the available empirical research is unclear as to whether tort reform substantially increases physician supply.

Although support for the proponents’ arguments on tort reform may lack a significant empirical basis, in one area the findings have been clear: caps do reduce the size and number of insurance payouts. Concerning the issue of payout size, Yoon’s 2010 study on tort reform in Alabama demonstrated that adoption of a damages cap reduced insurance payouts by about \$20,000, a significant difference compared to nearby control states.²⁵⁰ Yoon also demonstrated that the nullification of the Alabama damages cap in 1995 resulted in an increase in insurance payouts, affirming the effect.²⁵¹ In their 2013 study, Paik et al. also found a connection: adoption of damages caps resulted in a 23 percent reduction of the average payout and a 41 percent reduction in payout-per-physician.²⁵² On the issue of the number of paid claims, research demonstrates between a 10 percent and 38.5 percent decrease after adoption of tort reform.²⁵³

Even with this demonstrated change, however, there is a grey cloud behind the silver lining. While insurance payouts have been declining,

245. Sloan & Shadle, *supra* note 120, at 488; *see also supra* text accompanying note 120.

246. Paik et al., *supra* note 122, at 181–82.

247. *Id.* at 173, 203; *see also supra* text accompanying notes 122–26.

248. *See supra* note 131 and accompanying text.

249. *See supra* note 130 and accompanying text.

250. *See supra* text accompanying notes 89–93.

251. *See* Yoon, *supra* note 89, at 216–20.

252. *See* Paik et al., *supra* note 11, at 627 fig.7.

253. *See supra* text accompanying notes 103–06.

these reductions do not necessarily result in the lowering of insurance premiums.²⁵⁴ Several studies from the 2000s support the finding that tort reform lowers insurance premiums, but vary widely on the size of the effect.²⁵⁵ On the other hand, Hunter and Doroshow's 2002 study rejected those studies' findings that insurance premiums lower in response to tort reform.²⁵⁶ Instead, they found that states with "mid-range" reforms had *higher* insurance rates than states with no reform at all.²⁵⁷ Considering the findings of these contradictory studies, the effect of tort reform on insurance premiums remains unresolved, especially when compared to studies demonstrating caps' effect on damages.

By examining the proposed benefits of tort reform in light of the current state of empirical research, we conclude that many of the benefits claimed by proponents of tort reform are not matched by the current state of empirical research, while others have mixed support.

B. Cost/Harms Analysis: Doubling-Down Effect and Barriers to Personal Injury Compensation

If tort reform had some benefits but no drawbacks, it would remain—on cost-benefit terms alone—a beneficial modification to our judicial system. Instead, we believe that empirical research demonstrates that even with some benefits as discussed *supra* in Section IV.A, the negative impacts of tort reform are significant. The "doubling-down" effect—adding additional reductions in case filings on top of reductions in all states—has the effect of increasing undercompensation for medical negligence. On a more detailed level, though, we see that tort reform lowers compensation through two separate mechanisms: under-filing of claims after injury, and under-payment for claims proven to be valid.

We begin with a discussion of the problem of undercompensation through lawsuit deterrence. By analyzing New York data on claims filed and a representative sample of hospital patients, Paul Weiler and his colleagues assessed the rate of filing of lawsuits after injury.²⁵⁸ By assessing individual patient records, Weiler and his colleagues found that only one out of eight negligent injuries resulted in a claim; and, of those claims filed, only half resulted in payment.²⁵⁹ These results are consistent with a prior study by Patricia Danzon, who studied California data on injuries and claims for her work *Medical Malpractice: Theory,*

254. See *supra* text accompanying notes 109, 113–16.

255. See *supra* text accompanying notes 110–12.

256. See HUNTER & DOROSHOW, *supra* note 113, at 17–18.

257. *Id.* at 17.

258. WEILER ET AL., *supra* note 3, at 33–71.

259. *Id.* at 70–71.

*Evidence and Public Policy.*²⁶⁰ She found that the number of claims after negligent injury, less than one in ten injuries, was lower than Weiler's calculation.²⁶¹ In addition, she found that of those claims, only 40 percent received compensation for the plaintiff.²⁶² If we consider these stark data, which demonstrate that a significant number of negligent injuries never result in claims, and consider that we have found a "doubling-down" effect of tort reform, beyond the baseline reductions in all states,²⁶³ the problem of undercompensation for negligent injury is getting worse, especially with damages caps.

We also believe there is a significant problem of underpayment for filed claims. Damages caps act to undercompensate injuries by reducing the compensatory damages available for a valid claim.²⁶⁴ The effect can be severe in certain cases. A RAND corporation study from 2004 found a 30 percent reduction in total damages in California due to a noneconomic damages cap.²⁶⁵ The same year, Studdert et al. analyzed California's caps, finding that they reduced total damages awarded by 34 percent.²⁶⁶ More importantly, Studdert and his colleagues also found another problem with caps: they result in undercompensation for the most severely injured claimants.²⁶⁷ Hyman et al. found a similar result analyzing Texas data: the most severely injured claimants are the most undercompensated for their injuries after damages caps reduce claims.²⁶⁸ For most of these studies, the researchers evaluated the effect of caps on those claims already found to be meritorious.²⁶⁹ If Studdert, Hyman, and their colleagues are correct, damages caps reduce compensation by undercompensating the most gravely injured, even after their claims have been proven. Considering these undercompensation issues in light of the "doubling-down" effect of tort reform we have discussed, we believe that tort reform causes significant harms by creating a suboptimal distribution system for those most seriously injured. Sloan and Chepke may have put

260. DANZON, *supra* note 3, at 18.

261. *Id.* at 19.

262. *Id.*

263. See *supra* Figure 11 (depicting downward effect beyond lowered baseline).

264. See generally Michelle M. Mello et al., *The New Medical Malpractice Crisis*, 348 NEW ENG. J. MED. 2281, 2283 (2003) ("There is reasonable evidence that some of these measures, most notably damages caps, do reduce payouts, albeit at the price of undercompensating the most gravely injured patients." (endnotes omitted)); Paik et al., *supra* note 11, at 16.

265. PACE ET AL., *supra* note 52, at xx.

266. Studdert et al., *supra* note 50, at 58.

267. *Id.* at 63.

268. Hyman et al., *supra* note 62, at 382 (finding a statistically-significant increase in effect of damages cap for cases involving death).

269. Regarding the methodology of each study, see *supra* text accompanying notes 50, 53 (measuring reported jury verdicts).

it more bluntly but were right when—considering the underclaiming and undercompensation problems exacerbated by tort reform—they said: “if there is any benefit to caps, it is mainly in redistributing income from injury victims and their attorneys to health care providers.”²⁷⁰

If only the undercompensation issue were the sole negative effect of tort reform. Concurrent with the general problem of undercompensation, there is significant research evaluating the discriminatory effects of caps on certain population groups. The RAND study from 2004 found that, in addition to the 30 percent reduction in plaintiffs’ damages, those reductions disproportionately affect female and elderly claimants.²⁷¹ Using reported jury verdicts, they found that women plaintiffs had a median reduction of their awards of 34 percent compared to 25 percent for men.²⁷² As for the elderly, 67 percent of injured plaintiffs over age 65 had a reduction in their award, compared to 41 percent for plaintiffs of all ages.²⁷³

RAND is not the only study to report this finding. In their 2009 study of Texas data, Hyman et al. found that their cohort of elderly plaintiffs had an aggregate reduction in their verdict amount of 51 percent compared to 37 percent for other adults, with a per-claim mean of 23 percent compared to 19 percent for other adults.²⁷⁴ Finally, Lucinda Finley’s 2004 study of California tort reform from the *Emory Law Journal* also found a disparate impact on both women and the elderly.²⁷⁵ When assessing plaintiff’s recoveries in light of California’s damages cap, Finley found that the cap reduced the average compensatory damages awarded to female plaintiffs by over 48 percent and the median award by 57 percent.²⁷⁶ This compares to 40 percent and 31 percent for male plaintiffs, respectively.²⁷⁷ For plaintiffs over age 65, the average reduction was 34.6 percent, also above the median for non-elderly males.²⁷⁸ To Finley, the effect of damages caps is nothing short of a “form of discrimination against women [that] contribute[s] to unequal access to justice or fair compensation for women.”²⁷⁹ Clearly, the empirical research supports the conclusion that certain plaintiff groups are more likely to feel the effects of damages caps than others,

270. SLOAN & CHEPKE, *supra* note 1, at 309–10.

271. PACE ET AL., *supra* note 52, at xxiii.

272. *Id.*

273. *Id.* at 20 tbl.3.1.

274. Hyman et al., *supra* note 62, at 381. Hyman’s data sample did not allow for evaluation of gender discrimination. *Id.*

275. Finley, *supra* note 55, at 1263.

276. *Id.* at 1285.

277. *Id.*

278. *Id.* at 1313.

279. *Id.* at 1266.

and we believe this discriminatory effect is an additional negative externality of damages caps.

The undercompensation of plaintiffs' claims may serve as one major problem effect of tort reform, and by itself has significant problematic repercussions. We also believe, as Finley did, that in a broader sense the undercompensation problem undermines a core principle of our legal system, that of equal justice under the law. Caps can also affect certain plaintiffs' ability to retain an advocate for their cause.

When attorneys take into account tort reform measures that reduce awards and fees, the attorneys must decline cases they otherwise might accept. In the RAND analysis, for example, the effect of California's tort reform measures of the 1970s was to reduce fees by 60 percent from what they would be otherwise.²⁸⁰ These fee reductions must, as a simple matter of firm economics, affect the willingness of contingency fee attorneys to accept cases. Daniels and Martin found that exact effect in Texas, where over 90 percent of attorneys lamented the effect of tort reform on their practices.²⁸¹ In fact, the effect of damages caps may affect the willingness of plaintiffs' attorneys to take high-value claims more than other claims, said Steven Garber and his colleagues, because those cases are the most likely to be capped.²⁸² If so, damages caps have not only a general negative effect on access to justice, by reducing attorneys willing to accept contingency work, but also the more specific and perverse effect of reducing the ability of the most severely injured claimants to get representation. We believe, as Finley, Garber et al., and Daniels and Martin do, that these provisions inhibit the ideals of access to justice and equality before the law that are fundamental principles of justice.

One final externality bears discussion independent of undercompensating by under filing, undercompensating by damages reductions, the discriminatory effects of those cuts, and access to justice. Several studies have researched the effect of tort reform on litigation costs and discovered that, independent of their other effects, damages caps may also increase expenses. In their 2010 study, Ronen Avraham and Alvaro Bustos found that litigation expenses might be larger in states in which damages caps exist but are subject to further review or potential

280. PACE ET AL., *supra* note 52, at xxiv fig.S.1, 37.

281. Daniels & Martin, *supra* note 69, at 1239 tbl.2. They concluded that this has an effect on access to justice because there is an illusory remedy in law if a claimant cannot gain an advocate for representation. See *supra* text accompanying note 73.

282. Garber et al., *supra* note 74, at 682. Garber and his coauthors believe that these provisions have a specific effect on access to justice. *Id.*

nullification.²⁸³ Considering that constitutional challenges to tort reform are commonplace and often successful,²⁸⁴ the study suggests that most states adopting damages caps will experience a period of increased litigation expenses. A study of emergency department malpractice claims from the same year, 2010, provides support for Avraham's finding.²⁸⁵ In their analysis of emergency room claims from 1985–2007, Brown et al. discovered that even when total claims and paid claims have declined over the decades studied, litigation expenses have more than doubled with an overall increase of 137 percent.²⁸⁶ Even in an era of significant tort reform, then, litigation expenses have continued to rise.

When we began our cost analysis discussion, we affirmed that even with limited benefits, tort reform could be beneficial if the drawbacks were limited. We believe that in light of the significant negative externalities of tort reform, the cost-benefit analysis results in our conclusion that tort reform is not a beneficial modification to our judicial system.

C. Policy Implications

Tort reform was intended as a cure-all for problems with medical malpractice litigation: to stop defensive medicine, to lower insurance costs, and to attract more health care providers to a more competitive state environment for practice. Yet empirical research now demonstrates that of these proposed benefits, the only clear effect has been to lower insurance payouts, whether in damages or by non-lawsuit claims.²⁸⁷ But to achieve those limited benefits, significant negative consequences have arisen. Our research shows that damages caps reduce claimants' filings, but do so in the context of a reducing overall level of claims in all states. This "doubling-down" effect radically lowers filings, increasing undercompensation for injuries caused by medical negligence.²⁸⁸ Considering these effects are more pronounced for female, elderly, or

283. Ronen Avraham & Alvaro Bustos, *The Unexpected Effects of Caps in Non-Economic Damages*, 30 INT'L REV. L. & ECON. 291, 293, 299 (2010) (finding that for states with possible challenges to tort caps, litigation expenses increase as compared to other states).

284. See, e.g., *Ray v. Anesthesia Assocs. of Mobile, P.C.*, 674 So. 2d 525, 526 (Ala. 1995) (overturning cap in Alabama); *Lebron v. Gottlieb Mem'l Hosp.*, 930 N.E.2d 895, 899 (Ill. 2010) (overturning cap in Illinois). See generally Avraham, *supra* note 40.

285. Terrence W. Brown et al., *An Epidemiologic Study of Closed Emergency Department Malpractice Claims in a National Database of Physician Malpractice Insurers*, 17 ACAD. EMERGENCY MED. 553 (2010).

286. *Id.* at 558 (finding expenses increasing from \$12,693 in 1985 to \$30,810 in 2007, after adjusting for inflation, which corresponds to a 137% increase).

287. See *supra* Part IV.A.

288. See *supra* Part III.B.

severely injured claimants who file, and the caps negate the ability of many claimants to find an attorney to represent them, the negative externalities affect access to justice and may do so in a discriminatory fashion. When the law “doubles-down” on those citizens who are least able to defend themselves, and does so for the benefit of the few, we believe something must be done.

When a law has been enacted for one purpose but has become unmoored from that justification, it risks becoming illegitimate in the eyes of the governed. In 1980, Chief Justice Burger stated: “we cannot escape the reality that the law on occasion adheres to doctrinal concepts long after the reasons which gave them birth have disappeared and after experience suggest the need for change.”²⁸⁹ We believe damages caps for medical malpractice cases have now met this threshold.

We therefore suggest to the state legislators addressing the issue that they face a choice and have only two options. First, states with caps or those considering them can reject the idea of a “silver bullet” that can simultaneously solve insurance crises and medical litigation problems in one fell swoop, without affecting anyone’s legitimate interests.²⁹⁰ Because the effect of tort reform is to lower insurance payouts, and because it does so at significant costs to the elderly, to women, and to access to justice, we believe that this is the preferred option.

Our legislators need not abide by our advice, however, and states of course can maintain caps on damages recovery. But legislators must acknowledge that many benefits of tort reform initially suggested by the proponents of tort reform are not supported by current empirical research. Our legislators also must know, as the data accumulate, that the negative consequences of damages caps continue to collect. Beyond that cost-benefit calculation, our legislators must also recognize that if they choose to continue tort reform, they risk court intervention blocking their efforts due to the discriminatory effect of the policies.²⁹¹

D. Future Research

There are several areas of further research that build upon our work here, and we offer those as suggestions to further analyze the effect of tort reform. First, our study applies logistic fixed effect models to the analysis of the effect of damages caps in medical malpractice cases. We

289. *Trammel v. United States*, 445 U.S. 40, 48 (1980).

290. For one of many reform ideas, see WEILER, *supra* note 1, at ch. 3.

291. See Finley, *supra* note 55, at 1314 (“Legislatures must be attentive to discriminatory disparate impacts of damage cap laws on women and the elderly and should avoid enacting provisions that so starkly undermine our national ideals of equality and equal access to the civil justice system.”).

believe that the mathematical model would be equally effective in assessing any of the other types of tort reform.²⁹² The true limitation of assessing tort reform is the availability of data, as Eisenberg and others have noted. By assessing med mal caps, we believe we have a template for the reasoned consideration of each reform measure, and we intend to continue with those analyses as data permit.

We also believe that further research into whether reform was necessary in the first place would be fruitful. Neil Vidmar's 2005 analysis of tort filings in Illinois²⁹³ prior to tort reform in that state, and Bernard Black and his colleagues' work assessing the years leading to tort reform in Texas,²⁹⁴ could provide a template for additional study. Because the proponents of tort reform claim out-of-control filings necessitate litigation limits,²⁹⁵ a post hoc analysis of whether that assertion is valid could be replicated in other states.

A third possible area for further research involves analysis of the connection of insurance claim reduction to premiums. One of the very clear impacts of tort reform supported by recent empirical research is the conclusion that tort reform can reduce insurance claim payouts.²⁹⁶ Yet there is much less research about whether those insurance payout reductions translate into reductions in insurance premiums.²⁹⁷ Detailed analysis could affirm or refute that a benefit to carriers translates to benefits to the medical providers, but more research is clearly necessary.

Finally, we also believe that additional analysis of tort reform and its discriminatory impact will be critically important to the future debate of the issue. The work of Finley, Hyman et al., and the RAND Institute for Civil Justice shows that the effect of tort reform falls squarely on women, the elderly, and the most seriously injured claimants; future research could expand their findings to other reform methodologies, or confirm their work using other datasets. Further delineation of the discriminatory effect of tort reform would be essential to those seeking to overturn these laws through equal protection challenges.

292. For a list of other tort reform methods, see *supra* text accompanying notes 40–46.

293. Vidmar, *supra* note 3, at 340; *supra* text accompanying notes 132–37.

294. Black et al., *supra* note 3, at 207; *supra* text accompanying notes 138–42.

295. See *supra* text accompanying notes 24, 26, 32–34, 39.

296. PACE ET AL., *supra* note 52, at 35; Hyman et al., *supra* note 62, at 45; Studdert et al., *supra* note 50, at 58; see also *supra* text accompanying notes 50–65.

297. Regarding the current research on insurance premiums and tort reform, see *supra* text accompanying notes 106–16.

V. CONCLUSION

Legislative adoption of tort reform since the 1970s often came as a response to crises in insurance markets, but occurred without prospective research into its effects. Only later did empiricists begin to assess the effect of these limitations on plaintiffs in personal injury cases. As many researchers have done, we decided to explore those effects empirically, by analyzing changes in case filings in response to damages caps in medical negligence cases. When we collected data from 1992 to 2006, we found that the adoption of a med mal cap results in a reduction of med mal filings of between 13 percent and 23 percent. We can confirm the finding by assessing the effect of a cap's nullification, like what happened in Alabama in 1995. Using separate state data, we found that when a cap is invalidated, there is a corresponding increase in med mal filings, which confirms the impact of the cap on filing decisions. When our finding is added to recent research showing that all states have experienced a decline in med mal filings in the last decade, we believe tort reform “doubles-down” on claimants, meaning there is a background reduction considering larger non-statutory change, and a second reduction due to direct tort reform.

In the context of the debate over tort reform, we believe these results provide significant force to the rebalancing of cost-benefit assessments of tort reform. While the benefits of reform have been oversold, the costs continue to rise. As the “doubling-down” effect continues to further reduce filings, we believe damages caps no longer make sense. Therefore, we suggest to state legislators that they have two options: continue with the current course knowing that if they do not change, courts may intervene; or, reject tort reform as the panacea to solve insurance crises and medical litigation problems and move forward elsewhere.
